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Science Review

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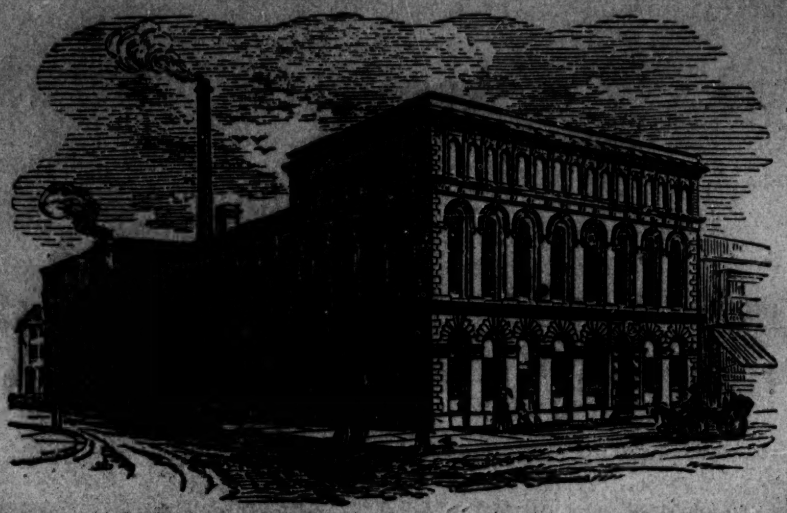

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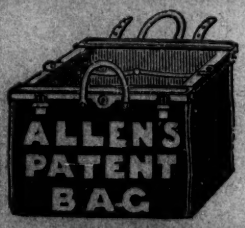

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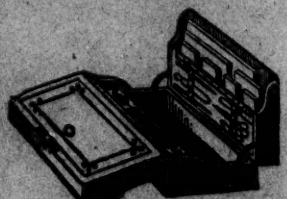

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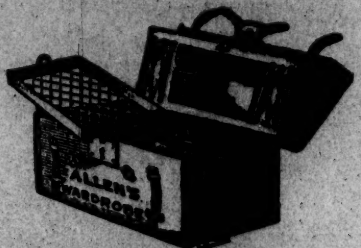

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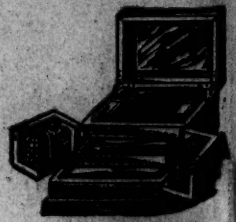

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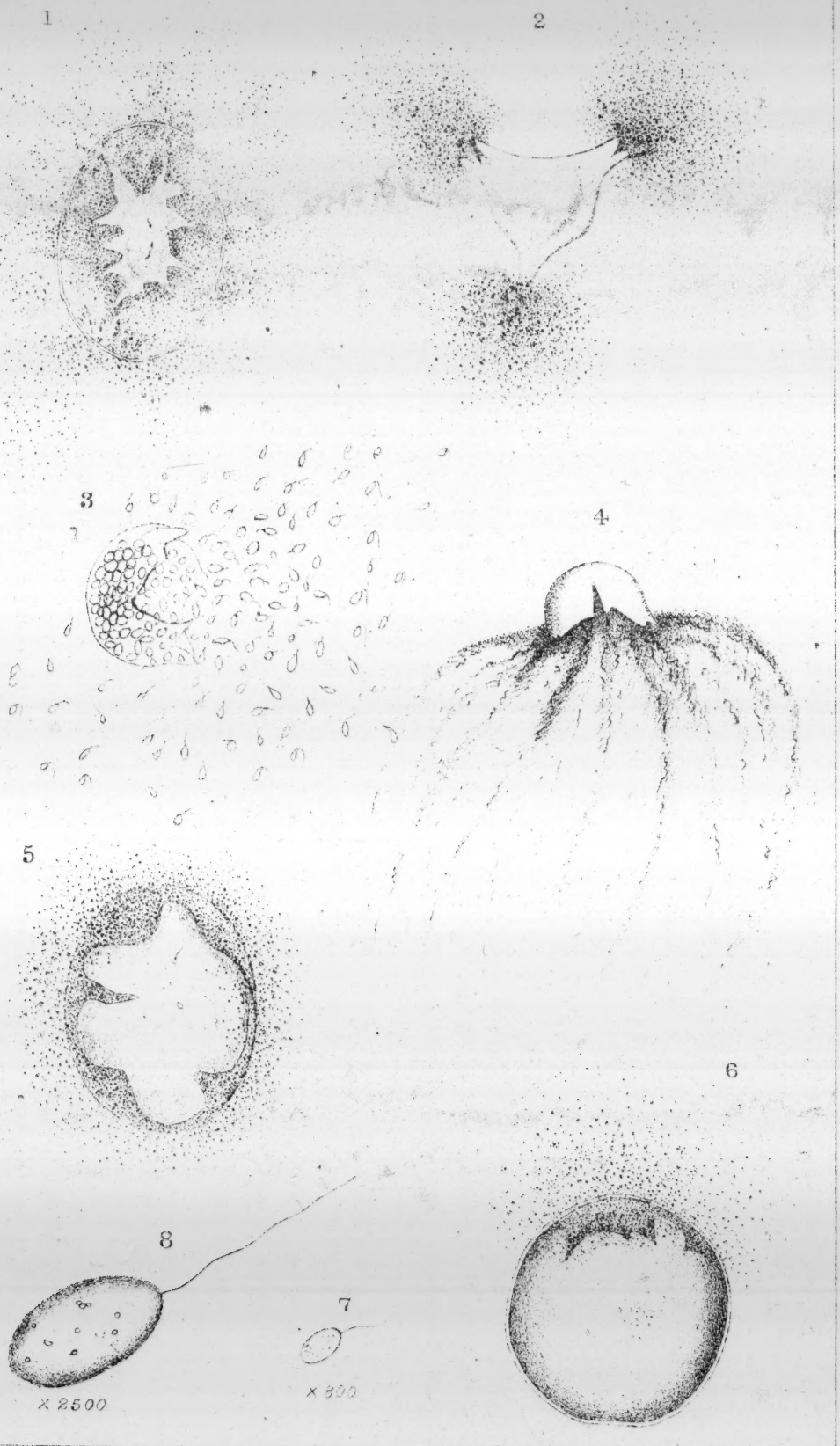

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Is there spontaneous generation?

PROFESSOR TYNDALL'S EXPERIMENTS ON SPONTANEOUS GENERATION, AND DR. BASTIAN'S POSITION.

By the Rev. W. H. DALLINGER, V.P.R.M.S.

[PLATE CXXXIII.]

IN the present position of Biological Science in relation to this important and interesting question, any positive results which have a definite bearing on the difficulties of the subject, and point hopefully to new methods of research, must be warmly welcomed. Professor Tyndall's beautiful series of experiments "On the Optical Deportment of the Atmosphere in reference to the Phenomena of Putrefaction and Infection" are precisely of this class, and will give new impulse and direction to all unbiassed labour. It is to be regretted when, in a matter so purely one of rigid science as this is, impassioned controversy is suffered to have any place. It fails utterly of its intended purpose, and simply hinders and delays the final issue. There are few but will have admired the animation, courage, and resolution manifested by Dr. Bastian in the discussion of this question during the last five years; but those who have been most capable of understanding the method, nature, and object of his experiments, and the general drift of his reasoning, are those who most earnestly disavow the perhaps unconscious, but nevertheless too palpable, advocacy of a *thesis* which his writings so freely display.

Dr. Bastian's position in relation to the origin of minute organic forms has, at the outset, the immense disadvantage of being adverse to the whole analogical teaching of nature, down to the uttermost depths of minuteness, *where our knowledge is accurate and sound.* Wherever science has put down the landmarks of possession, and is not dealing with the disputable territory of hypothesis, it is absolutely known that at some period in the cycle of development the lowliest organisms are

dependent for their propagation upon what we can only look upon as genetic products.

Manifestly, then, it must be weighty—nay, unequivocal and even irresistible—evidence that will induce the philosophical Biologist to conclude that nature's otherwise universal method is changed, in the outmost fringe of organised being. Mere reasoning could never accomplish this. It must be hard, defiant fact, which none can gainsay. But verily no such facts—nor even their most distant forecasts—are before us. The profound difficulties which bristle round the enquiry on every hand are prominent signals for caution; while the uncertainty aud incompetency of the methods hitherto employed, and their conflict of results, is alive with meaning. Indeed, we are dealing with organisms so minute as to elude all but our best optical appliances; and the accurate and correct interpretation of the details they enable us to discover requires the practice and experience of years. Of the developmental history of these organisms themselves, we know from actual observation almost nothing with certainty; and the little we do know from such careful and patient observers as Cohn, Billroth, Ray, Lankester and others, is so complex and conflicting as to demonstrate the necessity of years of patient experiment and skilled research; and to plainly tell us of our ignorance of this minute and wonderful group of organic forms. And yet, forsooth, we are asked, upon the conflicting testimony of a multiplicity of boiled infusions, yielding often even in the same hands uncertain results, and in different hands conflicting ones, to believe that organic nature—whose method of reproduction is the same to the very limits of *certain* knowledge—changes its method in this uncertain and cloudy region.

Of course to "spontaneous generation" as a mode of vital reproduction there can be no *à priori* objection. Let us have it by all means, if it be a fact in nature; but not on any other terms. Is it reasonable to suppose that such men as Darwin, and Huxley, and Tyndall, and Burdon Sanderson, and Cohn, and Billroth, and Lankester, would shrink from "spontaneous generation" because of the "consequences" to which, strangely enough, it is by some supposed to lead? The very thought admits of nothing but ridicule. And yet Dr. Bastian is displeased with Darwin * because he has not definitely determined whether all living things originated in one primordial germ, or originated spontaneously in multitudinous centres scattered over the earth's surface. Both Huxley and Tyndall are in effect charged with grave inconsistency,† because, while they admit the origin of all vital forms by evolution, they

* "Evolution and the Origin of Life," pp. 13–17. † Ibid. pp. 15–16.

yet declare that they have never *seen* an instance of "spontaneous generation" of organised forms. It is asked "Why should men of such acknowledged eminence in matters of philosophy and science as Mr. Herbert Spencer and Professor Huxley promulgate a notion which seems to involve an arbitrary infringement of the uniformity of nature?" I dare not answer for them; but for myself I answer, Because the facts as presented to them on the subject—as well known to them as to Dr. Bastian, and we may venture to say as well considered—do not appear to involve the "arbitrary infringement" of nature's uniformity of which Dr. Bastian speaks. If these admittedly competent and proverbially fearless men could be led by facts to see that their teaching promulgated an "arbitrary infringement" of nature's method, is it rational to suppose that they would persist in it another hour? The very position, therefore, of the leading biologists of the day in relation to the hypothesis of "spontaneous generation" is an authoritative declaration of the invalidity of the data on which it rests.

To Dr. Bastian, nevertheless, the "facts," such as they are, have carried a different conviction. But on analysis, that conviction is evidently not wholly formed upon the bare "facts." It is influenced and stimulated by a "philosophy" which, in short, is this:—Continuity in nature is the grand outcome of all modern research; but if you are to have this in a sense wide enough to include the organic world, you must have "spontaneous generation." Give up this, and continuous evolution is impossible; *therefore* abiogenesis *must be* a great truth.

Of course continuity in nature is a profound truth. Every careful and comprehensive student of modern biology will admit that. By Dr. Bastian's own showing, Huxley, Darwin, and Spencer are its most competent expositors. But they prefer not to be hasty. They decline to determine the exact manner or line of that continuity *until they have facts of a competent kind to guide them*. There may be lines of continuity infinitely more subtle than any the subtlest minds have even conceived. At least they decline to accept one, laid down, as it appears to them, not by nature, but by Dr. Bastian; and no believer in the evolution of living things, surely, is recreant of his creed who declines a similar surrender.

The largest difficulty surrounding the question of the mode of origin of septic organisms is that of discovering their life-cycle. By dealing with them in aggregations we run told and untold risks. The conflict of results by this means, in the most accomplished hands, employing the most refined methods during the past eighteen years, is a sufficient witness. Repetitions of experiments, and conflicting results, and explanations of the reason why; and so the cycle rolls. Of course important lessons

in biology are learned, but not *the* lesson. And yet by the teachings of this complex and doubtful method *alone* Dr. Bastian is content to accept "abiogenesis" as a great fact in nature.

To those who are best acquainted with the experimental history of the subject for the last twenty—but certainly for the last six—years this is the more remarkable. For the weight of evidence is certainly not only *not* in favour of "abiogenesis," but is in the strongest sense adverse to it. The most refined, delicate, and continuous researches all point to the existence of what are at present ultra-microscopic germs. This, indeed, is directly affirmed by the authors. A single and recent instance will suffice. After a remarkable series of experiments detailed before the Royal Society Dr. W. Roberts says: "The issue of the foregoing inquiry has been to confirm in the fullest manner the main propositions of the panspermic theory, and to establish the conclusion that *bacteria* and *torulæ*, when they do not proceed from visible parents like themselves, originate from invisible germs floating in the surrounding aërial and aqueous media."*

But further, this has been remarkably sustained by analogical evidence. There are putrefactive organisms that closely approximate to the bacteria in form, structure, and size. These are the "*monads*," or, as Professor Huxley doubtless more fitly names them, the *heteromita*.† They live side by side with the bacteria in the same putrescent mass, and certainly in the later stages of the disintegration of dead organic matter are the most active and powerful agents. From their greater size they present a more promising field for microscopical research than the bacteria themselves; and the life-history of some of these could be fully mastered. I long since felt that valuable aid might thus be rendered to the discovery of the nature of the bacteria. Armed with the best and most powerful appliances which the modern optician could supply, Dr. J. Drysdale and myself ventured on the work. The results are fully detailed elsewhere.‡ It need only be remarked here that the only hope of success was in *continuous* observation of the same form, in the same drop of fluid, under the highest powers. The secret, therefore, was to find a means of keeping the same drop under examination without evaporation. This we did. § The result was that patient work enabled us to completely unravel the life-history of six of these organisms. These life-cycles cannot be here recounted. Suffice it now to say that each of them multiplied enormously

* "Phil. Trans." 1874, p. 475.

† "Macmillan's Magazine," Feb. 1876, p. 379.

‡ "Monthly Micros. Journ." vols. x. xi. xii. and xiii.

§ Ibid, vol. xi. pp. 67-69.

by self-division (fission), but that the life-cycle in *each case* began and ended in *a distinct genetic product*—call them what we choose, spores, germs, or ova.

In Pl. CXXXIII. I have drawn from nature, in the six respective cases, the condition presented by each organism at the time of emitting its spore. Fig. 1 is the genetic product of an oval monad, with a pair of flagella; it rapidly increased by fission; then in a remarkable manner a pair blended, became *one* in the form of a sac, the sac burst and poured out, as the drawing portrays, innumerable spore, which were watched continuously until they were seen to develope into the parent condition. Fig. 2 gives a similar product of another form, different anatomically and in all the details of metamorphosis, but yet passing through the states of fission, blending into a sac, and (as seen) the emission of spore; which were again watched into the parent condition. Fig. 3 shows the direct genetic product of a third, but this sac did not contain spore, but *living young*, which swam forth at once upon the bursting of the sac; and by taking in pabulum at all points of the sarcode, rapidly grew to the parent size. In fig. 4 we have new features. The organism is oval, with one flagellum. It multiplies with enormous rapidity by *multiple* fission,* and then by distinct genetic union a sac is formed and spore emitted; but they are packed in a glairy fluid, and were so minute that at first our best powers failed to reveal them. But they were afterwards seen, and their full development traced. In figs. 5 and 6 we have the same products of the two last monads. In morphological detail they greatly differed from all the preceding ones, and from each other. But the spore-sacs were produced by the same means, and the exquisitely minute spore poured forth were traced through all their stages to the adult condition.

We have here, then, important indications of fact concerning the nearest allies of the bacteria: they develope from germs.

We have besides, the weight of the best experimental evidence pointing clearly to the existence of germs in the bacteria themselves. But the microscope has failed to *demonstrate* the latter. Its finest powers and finest methods failed to reach them.

Happily at this juncture Professor Tyndall has stepped in, and, with his accustomed brilliance and precision, has opened up the path we need. *He has presented us with a physical demonstration of the existence of immeasurably minute molecules of matter—utterly beyond the reach of the most powerful combination of lenses yet constructed—which are the indispensable precursors of bacteria in sterilised in-*

* "Monthly Micros. Journ." vol. xi. pp. 69–70.

*fusions.** In short, he has opened up a new and exact method, which must lead to a scientific determination of the existence and nature of the bacteria-germs. His beautiful experiments on the decomposition of vapours, and the formation of actinic clouds by light, led him to experiment on the floating matter of the air, and with what results is widely known. Confined and undisturbed air, however heavily charged with motes, becomes at length, by their deposition, absolutely clear, so that the path of the electric beam is invisible across it. From this, and associated indications, he acutely inferred "that the power of developing life by the air, and its power of scattering light, would be found to go hand in hand;" so that a beam of light sent across the air into which infusions might be placed and examined by the eye, rendered sensitive by darkness, might be utilised with the best results in determining the existence of bacteria-germs. To bring the idea to a practical result a number of chambers were constructed with glass fronts. At two opposite sides facing each other a couple of panes of glass were placed to serve as windows, through which the electric beam might pass. A small door was placed behind, and an ingenious device was arranged to enable a germ-tight pipette to have free lateral, as well as vertical, motion. Connection with the outer air was preserved by means of two narrow tubes inserted air-tight into the top of the chamber. The tubes were bent several times up and down, "so as to intercept and retain the particles carried by such feeble currents as changes of temperature might cause to set in between the outer and the inner air."

Into the bottom of the boxes were fitted air-tight large test-tubes, intended to contain the liquid to be exposed to the action of the moteless air.

"On September 10 the first case of this kind was closed. The passage of a concentrated beam across it showed the air within it to be laden with floating matter. On the 13th it was again examined. Before the beam entered, and after it quitted the case, its track was vivid in the air, but within the case it vanished. Three days quite sufficed to cause all the floating matter to be deposited on the sides and bottom, where it was retained by a coating of glycerine, with which the interior surface of the case had been purposely varnished. The test-tubes were then filled through the pipette, boiled for five minutes in a bath of brine or oil, and abandoned to the action of the moteless air."

In this way the air in its normal condition was freely supplied to the infusions, but of mechanically suspended matter it could

* "Nature," Jan. 27, 1876, p. 252; and Feb. 3, p. 268.

be demonstrated that there was none. And it was proved, with a clearness that admits of no quibble, that infusions of every kind, animal or vegetable, were absolutely free from putrefactive organisms. "In no single instance . . . did the air which had been proved moteless by the searching beam show itself to possess the least power of producing bacterial life or the associated phenomena of putrefaction." But portions of the same infusions exposed to the common air of the Royal Institution Laboratory at a continuous temperature of from 60° to 70° Fahr. fell invariably into putrefaction; and when the tubes containing them amounted to 600 in number not one of them escaped infection—they were all "infallibly smitten." Here is irresistible evidence that there is a direct relation between a mote-laden atmosphere and bacterial development. The whole series of Dr. Tyndall's exquisite experiments is simply an irrefragable affirmation of this truth. The presence of the physically demonstrated motes is as essential to the production, in a sterilised infusion, of septic organisms, as light is to actinic action. They cannot be made to appear without the precursive motes; they cannot be prevented from appearing if the motes be there. That these are the germs of bacteria by themselves, or associated with minute specks of matter, approximates to certainty in the proportion of hundreds of millions to one.

A beautiful illustration of the minuteness and multitude of the particles is given. Let clean gum mastic be dissolved in alcohol, and drop it into water; the mastic is precipitated and milkiness is produced. Gradually dilute the alcoholic solution, and a point is reached where the milkiness disappears, and by reflected light the liquid is of a bright cerulean hue. "It is in point of fact the colour of the sky, and is due to a similar cause—namely, the scattering of light by particles small in comparison to the size of the waves of light."

Examine this liquid with the highest microscopical power, and it appears as optically clear as distilled water. The mastic particles are almost infinite in number, and must crowd the entire field of the microscope; but they are as absolutely ultra-microscopic as though they had no existence. I have tested this with an exquisite $\frac{1}{50}$ of Powell and Lealand's, employed with a new and delicate mode of illumination for high powers,* and worked up to 15,000 diameters; but not the ghostliest semblance of such particles was seen. But at right angles to a luminous beam passing among these particles in the fluid "they discharge perfectly polarised light." "The optical deportment of the floating matter of the air proves it to be

* *Vide* "Monthly Micros. Journ." April 1876.

composed, in part, of particles of this excessively minute character," and it is among the finest of these ultra-microscopical particles that Professor Tyndall finds the sources of bacterial life. It is almost impossible to conceive a nearer approach to certainty concerning the nature of these minute particles than this. Their minuteness, their capability of being physically demonstrated, the absolute necessity of their presence to the origination of bacteria in sterilised infusions of any and every kind, taken in connection with what we *know* concerning the germs of the *heteromita* whose life-histories have been studied, render it simply inevitable that we have at length reached, what we are justified in believing to be, a genetic product of the bacteria through which their continuation as organisms is preserved. When first I saw the simplicity and beauty of this method, it struck me that its applicability as a test in reference to germs—*known to be such*—would have considerable collateral weight; and a method of employing it was suggested by a fact in past experience.* I had in my possession a maceration of cod's head, which I had kept in use for eleven months. It had become a pulpy mass, and in the middle of January last it was comparatively free from bacteria, but swarmed with two monads—the fourth and sixth of the series described by my colleague and myself. To ascertain their exact condition, I watched them on the "continuous stage" for three consecutive days, and found that both forms were to be seen plentifully emitting spore. The maceration had become very short of moisture, which served my purpose. I subjected it to a dryer air with a higher temperature, and it was not very long in becoming a moist pulpy mass, with sufficient cohesiveness to be removed from the vessel; and in this condition it was placed in a heating chamber, which was slowly raised to a temperature of 150° Fahr., and kept at this for an hour. This was 10° Fahr. higher than Dr. Drysdale and myself had proved necessary to destroy absolutely every adult form. The baked mass now appeared cracked, porous, and flaky. In parts it was extremely friable, and with little pressure crumbled into almost impalpable powder; while by friction a very large proportion was reduced to the finest dust. To avoid all possibility of error this powder was again exposed in the heating chamber, spread over a plate of glass, to a temperature of 140° Fahr. for ten minutes—thus rendering the plea of mere desiccation impossible.

A chamber or box was now prepared precisely like Professor Tyndall's, except that there were no tubes to communicate with the outer air.

* *Vide* "Monthly Micros. Journ." vol. xii. pp. 262-3.

In the "Researches" on the life-history of monads we had proved that they could live, thrive, and multiply almost as well in Cohn's "nutritive fluid" as in the normal animal infusion. This fluid is composed of phosphate of potash, sulphate of magnesia, triple basic phosphate of lime, tartarate of ammonia, and distilled water. If these ingredients are all mingled the fluid becomes speedily charged with bacteria, unless hermetically sealed, and sometimes even then. We therefore keep the ammonia in a separate solution, mixing them when required.

A portion of the fine dust of the maceration was now taken and thoroughly scattered through the air of the prepared chamber. The condensed beam from an oxyhydrogen lime-light* was then sent through it. Its line of passage was far more brilliantly marked inside the chamber than in the outer air. It was deemed inexpedient to insert the fluids while such brilliant points were visible in the air, and four hours were suffered to elapse. The lime-light beam was still visible with perfect distinctness, but its path within the chamber was much less brilliant and more homogeneous than it was without. The fluids were then carefully mixed, and five small glass basins of the mixture were inserted. The whole was undisturbed for five days. At the expiration of that time the beam of the lime-light sent through the chamber was absolutely invisible, although perfectly clear in the open air on both sides of it.

The fluids were now withdrawn. Ten "dips" were taken out of each basin for microscopical examination. *In every "dip"* —that is, fifty in all—*one or other of the monads appeared, and were in a state of active fission*; and in twenty-seven of the "dips" both monads were found. Bacteria swarmed the field, which of course I fully expected.

I now took five *other* glass vessels, and inserted them with great care into the now moteless air of the chamber, and poured in, as before, fresh Cohn's fluid. They were exposed for another five days. On careful microscopical examination of seventy-five "dips" *not a single monad of either form appeared*; bacteria were feebly present, but of course no steps were taken to guard against these, and, as before, they were anticipated.

The air of the chamber was again impregnated with dust, as before suffered for a time to settle, and *these same vessels of fluid*, which had yielded negative results, were again placed in the chamber. At the expiration of five days they were again examined, *and one or other of the monads was found in every successive "dip."*

Now let it be observed that there can be no possible error as

* This was of course very much less capable of "searching" than the electric beam; but it served for the rougher end I had in view.

to the forms. They were the identical species of the maceration, with which I am as familiar as with a barn-door fowl. What, then, is the logic of these facts? Dr. Tyndall proves that bacteria only develope in sterilized infusions when the air around them is laden with motes of incalculable multitude and exquisite minuteness. Given the presence of these, and the development of bacteria is inevitable. The inference is that the motes are *germs*. The above experiments show, that in closely allied septic organisms, the germs of which have been demonstrated and their developments watched, if the dry *débris* of a maceration in which these forms are found be scattered in the air around a prepared fluid, and demonstrated by similar optical means, that the said organisms develope; but if the minute dust from the *débris* be optically proved to be *absent*, none of the monad forms appear. Here we do not hypothecate a germ, but we *know* that it exists; and its deportment in similar conditions is identical with that of the assumed bacterial germ. Do we need more irresistible evidence that the bacteria develope, not *de novo*, but from genetic products?

Evidently Dr. Bastian thinks we do. He tells us in effect that if Dr. Tyndall has not succeeded, others have, in seeing bacteria reappear in infusions that have been exposed to a boiling heat for five minutes. This is true; but not to the extent nor with the meaning Dr. Bastian claims. He furnishes a list in "Nature"* for example, of those who are supposed to have secured the results he insists on. But this list is, perhaps hastily, but in effect, most unjustly framed. It is not surprising to see strong protests from the investigators concerned.† The citing of Roberts, for example, or Lankester and Pode, or Pasteur or Schwann, is simply a meaningless exercitation to all but the ignorant. Stripped of all disguise, the number of cases of the appearance of bacteria in sealed infusion after five or ten minutes boiling is few and doubtful indeed. But still there *are* cases, and in one instance at least admirably attested; but they are confessedly exceptional in a high degree. Dr. Bastian, however, prefers to interpret nature from the exceptional flasks, and infer "spontaneous generation" rather than be guided by the cumulative and overwhelming evidence of the existence of bacterial germs, as the medium of their normal reproduction. This must mean either that he believes that these organisms originate *de novo as well as* by germs, which is a direct *petitio principii*; or else that he is incapable of seeing the force of the facts which render the existence of germs inevitable. From the conflicting evidence of his own writing it would almost appear that he endeavoured to maintain

* Feb. 10, 1876. † E. G., "Nature," Feb. 24, 1876, p. 324.

both these views. He has recently said, "Professor Tyndall's results, admirable as they may be in themselves, *are altogether collateral, and do not bear upon the main point at issue.*" * Surely the "main point at issue" is the mode of origin of bacteria, and we cannot get much nearer the origin of an organic form than by tracing it to a genetic product—a spore! This *was* originally Dr. Bastian's question—did bacteria originate *de novo*, or from parents? It is not so now. He says, "*The* question is, not what air does or does not contain, since I have long ago shown .... that boiled fluids can be made to putrefy and swarm with bacteria in closed flasks, from which air and whatever it may contain has been expelled." † The same reasoning also obtains in his communication to the "Lancet" ‡ and to "Nature." § The result is clear. The doctrine of "spontaneous generation" rests upon *exceptions* for its truth. In rare instances, and in special infusions, bacteria have appeared after prolonged boiling. After a careful sifting of the evidence, the meagreness of the testimony is striking. All that can be fairly taken at all, when justly weighed, if taken altogether, is not equal to the evidence given by Dr. Burdon Sanderson. ‖ But it is well known that, while admitting and publishing the facts, he ignores absolutely Dr. Bastian's inference. And surely this is the truer philosophy. Let it be granted that by means not now explicable, the germs of bacteria, destructible in filtered infusions at a boiling temperature, are feebly, and at times, able to survive a slight continuation of the boiling point in infusions containing solid particles without apparent injury, is not that a ground for enquiring the reason why, rather than for inferring "spontaneous generation?" If we can prove that in 99 cases out of 100 *actual germs* are destroyed at 212° F., but that, in exceptional circumstances, the remaining one case yields bacteria *after* exposure to 212° F. for some minutes, is not that a reason for inferring, and looking for, some protective influence upon the germ, rather than launching into an hypothesis of an new mode of origin?

That the medium in which minute organic forms are subjected to heat exerts an influence on their subsequent deportment I can abundantly prove. I am equally convinced that the death-point of bacteria germs hovers very near the boiling point of water—a conviction amply sustained by fact. This being so, the survival, as germs, of some few, amidst incalculable myriads, by some accidental protection, is surely possible. So that, indeed, all true work now should be a study of the *germ* and its

* "Times," Jan. 29, 1876. † Ibid.
‡ Feb. 5, 1876. § Feb. 10, 1876.
‖ "Nature," Jan. 9, 1873, vol. vii. and vol. viii.

properties, and a discovery by patient research of the life-history of the organism.

The valueless nature of mere temperature experiments on such organisms, as tests of their ability to survive, without a knowledge of their life-history, Dr. Bastian, *without knowing it*, has made sufficiently plain. He gives a brilliant illustration —styled by himself "typical"—of the futility of his own method. Consider the facts.

In our "Researches" on the monads, my colleague and myself made it a special point to institute a series of investigations on the points of temperature which the adults, and the spore, of each form studied could resist. The results were as unexpected as they were remarkable. Only the results can here be stated. Taking the spore-sacs of the several forms in the order in which our illustration gives them, the data are as follow—viz. fig. 1 survived after exposure to 250° F.; figs. 2 and 4, 300° F.; fig. 3 (which produced living young), 180° F.; figs. 5 and 6, 250° F. That is to say, the spore, *after the heating to the above-named temperatures*, were followed step by step until they reached the parent condition. The adults of each form were absolutely destroyed at from 130° to 140° F. Thus, if all the examples be taken together, it will be seen that on the average the spore have a capacity to resist heat better than the adult in the proportion of 11 to 6. This is surely important.

Now, until Dr. Bastian's promised "new results"* have appeared, I believe I am justified in affirming that the strongest cases on which even he relies for "spontaneous generation" are recorded on pp. 175–180 of his "Evolution and the Origin of Life." They are thus introduced:—"After this I may, perhaps, be deemed fully justified in quoting two *very typical* experiments for the further consideration of those who stave off the belief in spontaneous generation—either by relying on insufficient reasons for doubting the influence of boiling water, or because of their following Pasteur, Cohn, and others in supposing that certain peculiar bacteria germs are not killed except by a brief exposure to a heat of 227° or 230° F. For even if we could grant them these limits, of what avail would the concession be . . . . in the face of the following experiments?" The details of the experiments follow. They are alike in method, and we will concern ourselves only with the second. A strong infusion of common cress, with a few of the leaves and stalks of the plants, were inclosed in a flask, which was hermetically sealed while the fluid within was boiling. It was then introduced into a digester and gradually heated, and afterwards kept at a temperature of 270–275° F. for twenty minutes, and was

* *Vide* "Times," Jan. 29, 1876.

retained at a temperature, if the time of heating and cooling be considered, over 230° F. for one hour. This flask was opened after nine weeks. The reaction was acid; the odour was not striking. On microscopical examination with a $\frac{1}{12}$ inch objective "*there appeared more than a dozen very active monads.*"

Now, fortunately, Dr. Bastian has not only carefully measured and described these organisms, but he has drawn them, and they are reproduced on the frontispiece of the book. He describes them as the 1-4000th of an inch in diameter; they were provided with a long, rapidly moving lash (flagellum), by which granules were freely moved about. But, besides this, "*there were many smaller, motionless, tailless spherules, of different sizes, whose body-substances presented a similar appearance to that of the monads*—and of which they were in all probability earlier developmental forms." *

Now, by careful comparison, I find that this monad is no other than the "uniflagellate monad," which is the fourth in the series whose life-histories were studied by Dr. Drysdale and myself.† Figs. 7 and 8, Pl. CXXXIII., will help to make this clear, where fig. 7 is an exact rendering of Dr. Bastian's monad magnified 800 diameters; and fig. 8 is a drawing of the "uniflagellate monad" described by my colleague and myself, magnified 2,500 diameters. We describe it thus:—"Its exterior form is extremely simple, being ovoid, with a single flagellum. Its long diameter never exceeds the 1-4000th part of an inch" in length.‡ Now, from a very prolonged and careful study of these organisms, I am convinced that Dr. Bastian's form and ours are absolutely identical. But to make the thing simply irresistible we have further and final evidence. One of the metamorphoses of this monad on its passage to multiple fission is that it loses its flagellum, and becomes precisely what Dr. Bastian saw all around—a motionless spherule."§ These little bodies are less in diameter than the active monad, and of precisely the same structure. The identity is thus complete. The evidence is as full as may be; the monad Dr. Bastian saw was the one whose life-history was fully worked out. As usual, it multiplies by fission, but the fission is multiple. It then passes to a sac-like condition, resulting from the uniting together or fusion of two individuals. This sac becomes still and bursts, as seen in fig. 4, pouring out spore that taxed our highest powers and closest watching. The spore of only two of the monads studied survived after exposure to a temperature of 300° F. *This is one of them.*

Now, Dr. Bastian says, "A drop of the fluid containing several

* "Evolution," p. 178. † "Monthly Micros. Journ." vol. xi. p. 69, *et seq.*
‡ P. 69, ibid. § P. 69, ibid.

of these active monads was placed for about five minutes on a glass slip in a water oven, maintained at a temperature of 140° F. All the movements of the monads ceased from that time, and they never afterwards showed any signs of life."* *This is precisely our experience.* But now mark the reasoning. This monad was killed at 140° F., *but it was found in an infusion that had been heated up to* 275° F.; THEREFORE *it must have originated de novo.*

But it has been shown that the monad has germs, and that these have a power of resisting heat up to 300° F.—that is to say, 25° F. higher than that to which Dr. Bastian's infusion *was* exposed—and *therefore*, by the logic of facts, the monads found were not a result of "spontaneous generation," *but were the natural outcome of a genetic product contained in the infusion, and which the heat employed could not destroy.*

We need no stronger proof of the futility of reasoning concerning the thermal death-point of a minute organism where developmental history is wholly unknown. Yet so confident is our experimenter of his result that he says: "Nothing that has yet been alleged, by way of objection to the admission of spontaneous generation as an everyday fact, at all effects such experiments as these. The shortest way out of the difficulty would, therefore, be to doubt the facts." But I think I have shown a still shorter way "out of the difficulty," and that, without the discourtesy of doubting Dr. Bastian's experimental "facts."

The truth, then, is that Dr. Bastian had no real knowledge of the monad; but he argued as if he had. Hence assumed premises led to a false and fatal conclusion.

He is simply repeating this in his latest attitude in reference to the question of the mode of origin of bacteria. Compelled to yield all else, he throws up a rampart round his exceptional flasks, and declares "spontaneous generation" to be impregnable—an inviolable law of nature. Dr. Tyndall is plainly told that his knowledge is insufficient, that he has mistaken the meaning of the question, and that his mode of treating it is "laughable;" † and all this arises from the fact that Professor Tyndall dealt with the question of *the mode of origin* of bacteria generally; whereas, to have pleased Dr. Bastian, he ought to have explained some exceptional conditions to which he now points—the exceptions being more important than the rule!

What are the facts?

I. Dr. Tyndall has proved, in connection with a host of others, but in a more definite and precise manner, that in *filtered infusions* five minutes' boiling does kill every form of bacteria.

* "Evolution and the Origin of Life," p. 179.

† "Lancet," Feb. 5, 1876; and "Brit. Med. Journ." Feb. 5, 1876.

II. He has further shown that they are propagated by demonstrable germs *only*, in such infusions ; and

III. This fact removes the probability of their spontaneous generation to an almost infinite distance.

As to the development of bacteria in infusions charged with solid matter, precise experiment of a sufficiently comprehensive character has yet to be made on them, in relation to the demonstrated germs. Meantime, shall we accept "spontaneous generation" on such ground as its strongest advocate has now to offer, and ignore the vast chain of facts copiously attested and controlled, which are in perfect harmony with the known laws of the entire organic world? This, and nothing less than this, is what Dr. Bastian inculcates and demands.

EXPLANATION OF PLATE CXXXIII.

FIG. 1. Spore-sac of the cercomonad—the first in the series of Messrs. Dallinger and Drysdale's "Researches"—emitting spore.

FIGS. 2, 3, 4, 5, and 6. The same states of the five other monads of the series.

FIG. 7. A monad found by Dr. Bastian in an infusion, after heating up to 275° F., said to be spontaneously generated.

FIG. 8. The same monad as seen by Dallinger and Drysdale, and the spore of which (fig. 4) survives 300° F.

HEAT AND NOT LIGHT A MOTIVE POWER; OR, EXPERIMENTS WITH RADIOMETERS.

By H. A. CUNNINGTON.

THESE ingenious and beautiful little instruments were invented by Mr. Crookes. I believe he at first supposed that they showed "repulsion by radiation" of heat; since then he speaks of them as showing the "mechanical action of light," and has gone so far with his experiments as to *weigh* a ray of light, proving that the *force* of the light our earth receives from the sun is equal to fifty-seven tons per square mile. I have been experimenting for nearly three months with some of the radiometers made by Dr. Geissler, and obtained through Mr. Browning, who has kindly placed at my disposal some of the best he has had. As these little instruments are not generally known, I will try and give a description of them before beginning an account of my experiments. Four little arms of equal length are fastened at right angles to one another, and attached to a socket of hard glass, in which the point of a needle works as a pivot. On the free end of each arm is fastened a vane, black on one side and white on the other, each one of the same size, and so placed that they shall be like a continuation of the arms, and be at right angles to the plane of revolution. These arms, vanes, socket, and needle are enclosed in a glass globe, with a hollow stem, which serves as the means of keeping the globe upright by fixing it in a hole in a short wooden stand. The air is exhausted from the globe by means of a Sprengel air-pump until the nearest approach to a perfect vacuum has been obtained. All my experiments seem to point to the conclusion that "heat" is the motive power, and not "light"; that is, if they can be considered as separate agencies. A theory of the action of the heat has been suggested to me by my brother, Mr. Cecil Cunnington, which seems to be able to account for all the phenomena I have as yet observed. I will not give it until after the account of my experiments, as it will then perhaps be more intelligible.

I have two radiometers exactly alike in make, with the exception that one, which I shall call A, has diamond-shaped vanes, of which the reflecting sides are not so effective as the absorbing sides; and in the other, B, the vanes are round, with very effective reflecting surfaces, but indifferent absorbing surfaces.

When A and B are placed, say, six inches from any source of light, as a candle or in daylight, the black discs in A are repelled much quicker than those in B—that is to say, the rotation is much quicker in A than in B. This difference would be partly caused by the difference in black area in the two shapes of the vanes, but other reasons for it will be mentioned hereafter. I shall class my experiments under two sections. Under the first, I will describe those in which heat acts *immediately* upon the globe of the radiometer; under the other, those in which *light* and *heat* act indirectly upon it. The experiments will make these definitions more intelligible.

I. I brought radiometer A into a dark room (without a fire), and lighted a small benzine lamp, placing it almost directly over the top of the globe, and about two feet from it. I turned down the flame until the vanes were only just visible. The light had no effect on them, and in fact had none when I afterwards turned it up to its full height. I then placed one finger on the side of the globe, much *nearer to a black than to a white* disc (for convenience I shall call one side of a vane the white, the other the black disc). The black was at once repelled, but not with sufficient force to allow the white disc on the next vane to pass the heated spot, and so bring the next black disc under its power. I then allowed the globe to cool, and placed my finger as before, only much nearer to a white than to a black disc. The white was repelled nearly as much as the black had been, but the next vane was not brought past the heated spot. In no way could I force a vane through an angle of 90°. With two fingers, one on each side of the globe, the effects were the same as when I used one finger. I then surrounded the globe completely with my warm hands; no rotation was produced. The discs moved backwards and forwards through an angle of about 40°. I then allowed the globe to cool—no motion. Bringing radiometer B into the same position, I placed my finger as before, nearer to a white disc. It was repelled through an angle of about 40°, but the next vane was not brought past the heated spot. On removing my finger, the black disc gradually *approached* the heated spot, and, as soon as it had passed it, the movement quickened, and complete rotation ensued. This backward movement, apparently resulting from repulsion of the white discs, I shall hereafter call the "reverse motion." As soon as the discs became

stationary, I placed my finger nearest to a black disc; it was at once repelled; the white approached, passed the warm spot, the next black was then repelled—thus continuous rotation was produced. I then removed my finger, and allowed the globe to cool. Friction soon overcame momentum, the vanes stopped, and "*reverse motion*" *immediately commenced.* With a finger on opposite sides of the globe, the same effects are produced, but with greater intensity. When perfectly still I turned the flame of the lamp down so low that I could not distinguish anything in the room, and then surrounded the globe with my warm hands. In about half a minute, suddenly turning up the flame, I found the vanes rotating rapidly (making 40 or 50 complete revolutions per minute), the black discs being repelled. Removing my hands, reverse motion ensued; and as soon as the vanes had become stationary, the temperature being equalised, I turned down the flame, so that there was only just enough light to enable me to see the vanes, and again placed my hands round the globe. The black discs were repelled, rotation quickened, reached its maximum, and then gradually decreased in velocity until the vanes were perfectly stationary. The little air inside having been raised to the same temperature as that of my hands, when they were removed rapid reverse action at once ensued. When the vanes again became stationary, I allowed two or three drops of sulphuric ether to fall upon the globe. The rapid evaporation, producing cold, immediately caused reverse motion, which continued for two or three minutes, when the vanes again became stationary, and the motion was reversed, the black discs being now repelled as the globe was receiving heat from the outside air. This experiment can be repeated for any number of times; the hotter the surrounding air, the greater the effect. It can be produced as well in the dark as in the light, and if a single drop of ether fall on the globe every four or five minutes, rotation—first in one direction, and then in the other—must continue until the temperature of the whole of the surrounding air be reduced to that degree of cold obtained by the rapid evaporation of the ether. Thus, with a perfect radiometer, unlimited supply of ether, and means of dropping it when required, motion may be produced which must practically be perpetual.

The next experiments were tried in daylight. I placed B in front of the fire for a few seconds. The black discs were repelled with such rapidity that I could not distinguish the vanes. On putting it quickly outside my window, where the thermometer stood three or four degrees below the freezing point, the rotation soon ceased, and reverse motion set in, at first with a velocity equal to thirty-three complete revolutions

per minute; this motion, gradually decreasing, continued for nearly ten minutes. These and many other similar experiments seem to point to the conclusion that when a radiometer is cooler than its surroundings, or, in other words, is receiving heat, the black discs are repelled; but if, on the other hand, it be warmer than its surroundings, and is giving out heat, the white will be repelled, or the black attracted.

By covering the globe with hot and cold glass shades alternately, I have made the discs revolve in whichever direction I wish. I have no doubt that all radiometers with reflecting and absorbing discs may be made to rotate in either direction, provided they are properly treated, some requiring greater differences in temperature than others. I find that my A, in order to obtain reverse motion, must be subjected to a change of nearly 40°, while B requires very much less. The latter is so sensitive that, when rotating under the influence of sunlight, reverse motion sets in when the window-blind is drawn down. The effect of hot or cold breath upon the glass is similar to the above; so one can *blow* the vanes round in either direction.

When A and B were placed on the chimney-piece, both the black discs were repelled under the influence of diffused sunlight; A with a velocity equal to three revolutions per minute, and B seven. Both were then covered with equally cold glass shades (at about freezing point). In five seconds B stopped and reversed motion (five revolutions per minute), stopping in about $1\frac{1}{2}$ minute, and then again repelling the black. A simply decreased in velocity for a minute or two, and then moved as quickly as before being covered with the shade. When a candle was placed seven inches from B, the black discs were repelled at the rate of eleven revolutions per minute. I then covered it with a cold glass shade. Motion ceased in thirty seconds, and reverse motion set in (against the candle-light), and continued for forty-five seconds, when it again stopped, and original motion ensued. I then placed A $10\frac{1}{2}$ inches from the same candle, which caused the black to be repelled with the same velocity (eleven revolutions per minute). When covered with the same glass shade, re-chilled to the same temperature, the rotation decreased to six revolutions per minute, and then gradually quickened as the shade became warmer. To cause the black to be repelled six revolutions per minute, I had to place the candle $15\frac{1}{2}$ inches from the globe.

I must now pass on to section II. The last-mentioned experiment comes partly under one section and partly under the other.

When a candle or any source of light is placed, say, 6 inches from the globe, the heat, having passed through 6

inches of air, is incapable of heating the residuum of air within the globe, and unless the radiant heat is very great, the glass is not appreciably warmed. I placed one candle about 12 inches from B, and found its light (or heat) capable of producing slow black repulsion. Eleven other candles were then lighted on the same side of the globe, and at the same distance from it; they produced quick rotation. Ten of them were then suddenly blown out; the vanes soon became stationary, and reverse motion set in, the black discs moving against the light of the two candles. Under other conditions, these two candles would have caused them to be repelled with a velocity equal to $1\frac{3}{4}$ revolution per minute. With three lighted candles, and then two of them blown out, the same effect was produced, though not with the same intensity. By raising or lowering the flame of a benzine lamp, rotation in either direction can be produced; the degree of reverse motion, I find, always depending on the degree of diminution of light. These effects cannot be produced with radiometer A.

Into a large glass jar, filled with a strong solution of alum, I placed A, the globe being surrounded with nearly three inches of the solution. When the solution was of the same temperature as the air of the room, I placed a lighted candle one inch from the jar, another exactly opposite, one on either side, and so increased the number to nine, but no movement of the discs was caused, except when the sixth candle was lighted. Then the discs moved through an angle of 135°, and again became stationary. The reason of this will presently be explained. When the tenth candle was lighted, the black discs were repelled with a velocity of $2\frac{1}{2}$ complete revolutions per minute; with the eleventh candle, $3\frac{1}{4}$; twelfth, 4; thirteenth, $4\frac{1}{2}$; fourteenth, 5; fifteenth, $6\frac{1}{4}$; sixteenth, $8\frac{1}{2}$. By gently stirring the top of the solution with a glass rod the movement was increased to $10\frac{3}{4}$ revolutions per minute. I find the action of light which has passed through alum solution much greater when the solution is not perfectly still. When the sixth candle was lighted an accidental shake given to the table produced a little motion in the liquid, and the discs moved as mentioned above. If the solution be stirred too violently, the colder liquid from the bottom is forced upwards, and rotation ceases. This can also be effected by allowing a little of the solution, chilled in a pipette, to run down the side of the globe. By the same means, a little of the solution when heated will produce repulsion of either disc, as in the experiment with a warm finger, already mentioned under section I.

I tried on B the effect of light after it had passed through different thicknesses of alum solution. The repulsion of the black was always much decreased, varying with the thickness

used. When the light of three candles passed through half an inch of the solution, slow rotation was produced. Two were blown out; the vanes stopped, and reverse motion ensued. The two candles were again lighted, and a short piece of magnesium ribbon burnt behind them. Rotation immediately quickened, and as soon as the ribbon came to an end, the vanes stopped, and reverse motion ensued. The same effects were produced when a thickness of 8 inches of the solution was used, but in a less degree. I find that whenever light, passing through an alum solution of whatever thickness, produces black repulsion, reverse motion begins as soon as the source of light is removed or in any way diminished. The more perfect the radiometer, the less this diminution need be. In a radiometer with bad reflecting surfaces, the black repulsion will only decrease in velocity. Motion may altogether cease if the degree of diminution of light be very great, but reverse motion cannot be produced. I find that the light of three candles, when passed through half an inch of the alum solution, and causing slow black repulsion in the radiometer, is capable of slightly heating the *blackened* globe of a differential thermometer, and causing a depression of the sulphuric acid equal to about one-thirtieth of an inch, the other globe being carefully sheltered from the light. I could not observe any depression when a greater thickness of alum solution was used.

I find that a source of light, however bright, cannot produce rotation of the vanes of a radiometer unless it is capable of raising the temperature of the residual air within the globe. Two or three experiments, I think, prove this. I placed B close to a given source of bright light; very rapid black repulsion at once ensued. Taking it from its stand, and heating the whole of the globe and stem over the flame of a spirit lamp until the glass was very hot, and the air inside raised to a like temperature, I quickly placed it in its original position, and found that the light had no effect, the vanes remaining perfectly stationary for more than a minute; when, the globe and internal air becoming cooler, black repulsion ensued. Wishing to produce this effect without sensibly heating the globe, I placed the stem of B in a large test tube filled with cold water. The bright light of a candle placed 3 inches from the globe produced quick rotation. I then heated the test tube with the flame from a spirit lamp, taking care that the hot external air should not ascend and warm the globe. As the water became warm, and heated the air in the stem of the globe, the hot internal air rose, the black repulsion became less and less, and when the water nearly boiled all motion ceased for a few seconds, and *reverse motion set in.* The air inside now being warmer than

the candle flame could make it, gave out heat instead of receiving it. I then tried the converse experiment. After counting the number of "black repelling" revolutions produced in one minute by the candle when the radiometer had obtained equal temperature with the surrounding air, I chilled the air in the top of the globe with "ether spray." The heat the internal air received from the black discs was thus quickly disposed of, and the candle was able to produce greater effect, which was apparent by the increase in the velocity of the rotation.

Wishing to somewhat vary this experiment, I fastened cotton wool tightly round A, until it was encased in a thickness of nearly an inch, leaving a part of the globe uncovered about the size of a half-crown piece, thus preventing to a great extent any radiation of heat from any part of the radiometer. I then covered it with a large glass jar, and on one side placed a solution of alum about half an inch thick, which prevented the radiant heat from nine candles warming the globe. These nine candles were thus placed about 16 inches from the radiometer; their light having to pass through the alum solution, the two glass sides of the vessel containing it, the glass jar and the glass of the globe, before it could affect the discs. They were of such a height that the more they burnt away the greater effect they ought to produce, as the more light could then pass through the different glasses and the solution, and through the opening in the cotton wool placed nearly opposite to them. This opening was so situated that the light should fall almost entirely on the *black* discs. Soon after the nine candles were lighted I noticed that they caused the black discs to be repelled with a velocity equal to $4\frac{1}{4}$ complete revolutions per minute. After burning for 15 minutes, the velocity had reduced to $3\frac{1}{4}$ revolutions, and in time, I have no doubt, would have almost ceased. Then suddenly taking off the cotton wool, the velocity increased to $5\frac{1}{8}$ revolutions, at which rate it remained without change. Four candles produced at first $2\frac{3}{8}$ revolutions, decreasing in 15 minutes to $1\frac{1}{2}$, and when the wool was removed immediately quickened to nearly 3. The great difficulty in these experiments is to prevent the loss of heat from the globe by radiation. If all is preserved, rotation, sooner or later, would probably cease.

I have reason to believe that moonlight is capable of repelling the black discs, and that without the use of a condensing lens or telescope. I find the experiment attended by many difficulties; but if the radiant heat from the observer's body at a distance of 8 feet from the radiometer is incapable of affecting the discs, when the temperature of the surrounding air is 10° below freezing point, then moonlight will cause the black discs to be slowly repelled at a

rate of about one complete revolution in two minutes. This was tried with my radiometer B. With A the velocity would be greater. According to the theory which has been suggested to me, the repulsion or attraction of the black discs is caused by the direct action of heat, and it seems to be able to account for all the phenomena I have as yet observed. The way in which it affects the discs is somewhat similar to that shown in the "Trevelyan rocker." Let us suppose a radiometer with good reflecting and absorbing discs to be at rest, a candle to be then placed at a distance of 6 inches from it; the light or heat from the flame, having crossed 5 inches of common air, is incapable of heating the residuum of air within the globe. It is arrested and absorbed by a black surface, which becomes warm, and heats the adjacent particles of air. They expand, and give the disc a push into the cooler air behind, which the reflecting, non-radiating surface was incapable of warming. The ordinary black repelling motion would be thus produced; the greater the heat the more the expansion, and the quicker the repulsion. Remove the source of heat, and chill the globe. The friction soon overcomes the momentum, and the discs are at rest; the air inside is warm; the black absorbs a little of the heat close to its surface; causes contraction of the air. A space is formed into which the disc is pushed by the hot air behind it—the backward or "reverse motion" would thus be caused.

This reverse motion can never be very great, as the (motive) heat is limited in quantity, and it is expended by gradual conversion into mechanical action, which has friction to overcome, and also is lost by radiation from the outside surface of the globe. It is evident that unless the reflecting surfaces are good, this backward motion could not be obtained, as both sides of the vanes would cool the adjacent air by absorption, and the difference between the two actions would not be sufficient to cause the rotation. This is why I can get little, if any, reverse motion in my radiometer A.

When heat is applied to the globe direct, as is done by touching it with the finger, &c., the action is somewhat different. The glass becomes warm, and it warms the air within, producing expansion, which repels whichever side of a vane is nearest to the warmest part of the globe. If this happens to be the white side, no rotation can be produced, because the continued application of heat repels the white, and the opposite black disc (on the next approaching vane) absorbs the heat, and has, as in the first case, a strong tendency to move in the opposite direction; but should it happen that a black disc is nearest to the warm glass, rotation immediately ensues. The black being repelled by the expanded air, and taking with it

into a cooler part of the globe enough heat (obtained by its absorptive power) to enable it to be further repelled, as in the way already described, the next black disc is thus brought under the influence of the warm glass, and the operation is repeated. If, when the white disc be first repelled, and no rotation produced, the finger be removed, and the globe chilled, the black gradually approaches the heated part of the globe, absorbing and radiating the heat, and so contracting the air, and making room for itself, as mentioned in the description of "reverse motion." I noticed that the black surfaces on my radiometer B were gradually becoming more *grey* every day; in fact, the black (lamp black, I believe) was coming off little by little—in places it had worn almost white. This was not the case with A, although it had done much more work. I at once thought that this was caused by the reverse motion, the black particles having been *sucked off* by the contracting air. I therefore placed B on its side, and so collected the black dust in one spot in the bottom of the stem of the globe, and carefully placed it upright again. I then heated it, and so caused the black discs to be repelled with great velocity, carefully noticing at the time, by the help of a magnifying lens, whether any black particles fell on to that side of the stem that I had cleared of them. Not one appeared until the reverse motion began. I was then soon able to count a score or more. I repeated this experiment two or three times, and always with the same effect. It must not be tried too often, as every time it makes the black discs less sensitive to the action of heat and light for further experiments.

I think one can compare the residuum of air within a radiometer to a piece of elastic stretched almost to its utmost, but far too strong to be *broken* by any "Sprengel air-pump."

I have sometimes used the word "heat," and sometimes "light" in describing these experiments. They cannot be considered as two separate agencies. All that is necessary to produce "black repulsion" in a radiometer is that one should use some part of the spectrum between the ultra red or heat rays, wherever they may begin, and the extreme luminous part. The action decreases rapidly from the red towards the blue. If the *ultra* red (or "*active* heat") rays be cut off by the intervention of an alum solution, the red rays (the others giving but very little help) are absorbed by the black surfaces, and a little heat is produced, which it seems causes the repulsion. If a source of light be used which is very deficient in red rays, the light, however intense, is hardly capable of moving the discs. I have used the electric spark from a "6-inch" induction coil, intensified by introducing Leyden jars into the secondary circuit, and allowed the continuous stream of sparks

to pass between platinum electrodes. This dazzling light, when concentrated by a condensing lens on to a black disc, had no effect whatever, although allowed to remain there for many minutes. The same spark, when passing between carbon electrodes, repelled the black, and produced *very* slow rotation. The carbon was made warm by the electric current, and the radiant heat doubtless produced the effect.

From the experiments I have described, I think we can deduce three leading principles which govern the movements of radiometers having *good* reflecting and absorbing discs.

I. When a radiometer is receiving light or heat, being at a lower temperature than its surroundings, repulsion of the black discs must ensue, and continue until the temperatures are equalised.

II. When a radiometer is radiating heat, being at a higher temperature than its surroundings, attraction of the black discs or the apparent repulsion of the white discs, must ensue, and continue until the temperatures are again equalised.

III. No source of light can produce repulsion of the black discs of a radiometer unless it is capable of raising the tem perature of the residual air within the globe.

RAILWAY TRAVELLING AND ELECTRICITY.

By W. PREECE, C.E.,

OF THE POST OFFICE TELEGRAPH DEPARTMENT.

PLATE CXXXIV.

TERRIBLE railway accidents like that at Abbot's Ripton excite the public interest with great intensity for periods of time varying with the magnitude of the disaster, and the amount of other stirring events occurring simultaneously. There is no sensation greater than that of a severe railway accident. It affects every one. We are all travellers by railway, and self-interest makes us read with horror and dismay of the death of *units* in a railway train, while we pursue our breakfast with comparative calmness during the recital of *hundreds* smothered to death in a colliery explosion, or sent to eternity in a watery grave. The sensation is aggravated by the public press, which gives such prominence to every detail of what is called "this railway butchery," while it leaves unchronicled the terrible slaughter that occurs daily in our midst from other causes. There are more people killed *every day* by accident than are killed *every year* from causes beyond their own control on the railways of the United Kingdom! Mr. John Bright said that a first-class carriage was the safest place to deposit oneself in, and a perusal of the following statistics will go far to support that opinion.

In 1873 no less than 17,246 persons met with violent deaths in England and Wales, which is an average of 750 per million, or 1 in 1,354, or 47 per day. The causes of these deaths are thus analysed:—

TABLE I.—VIOLENT DEATHS IN ENGLAND AND WALES FOR THE YEAR 1873.

Cause of Death.	No.
Injuries in mines	990
Mechanical injuries (not on railways or in mines)	6,070
Chemical injuries	2,784
Asphyxia	5,193
Violence (unclassified)	919
Railways	1,290

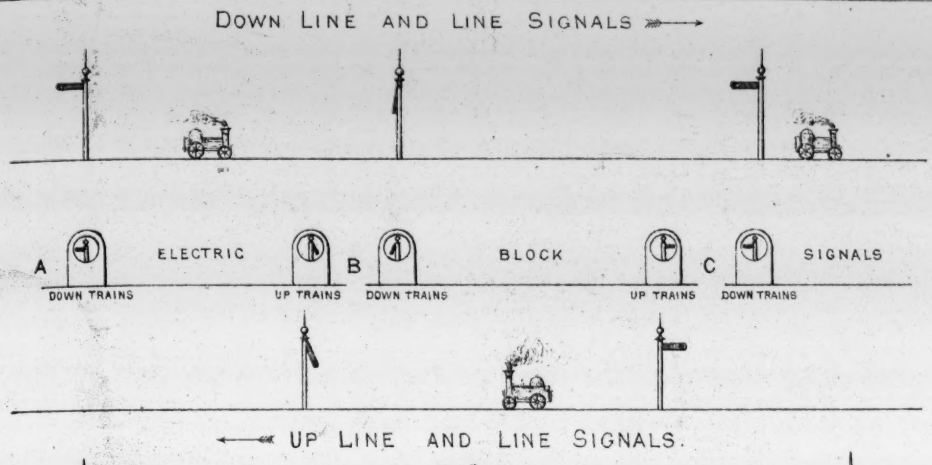


Fig. 1.

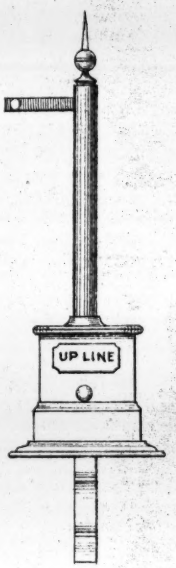


Fig. 2.

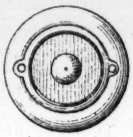


Fig. 5.

Fig. 4.

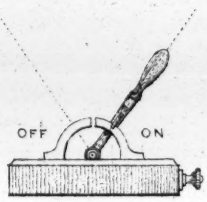
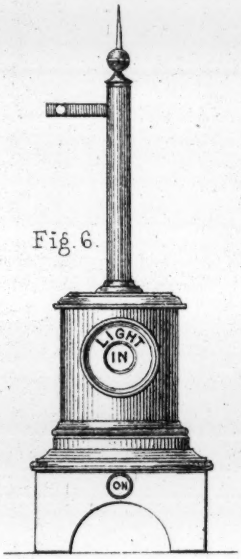


Fig. 3.

Fig. 6.



LIGHT AND ARM

Fig. 7.

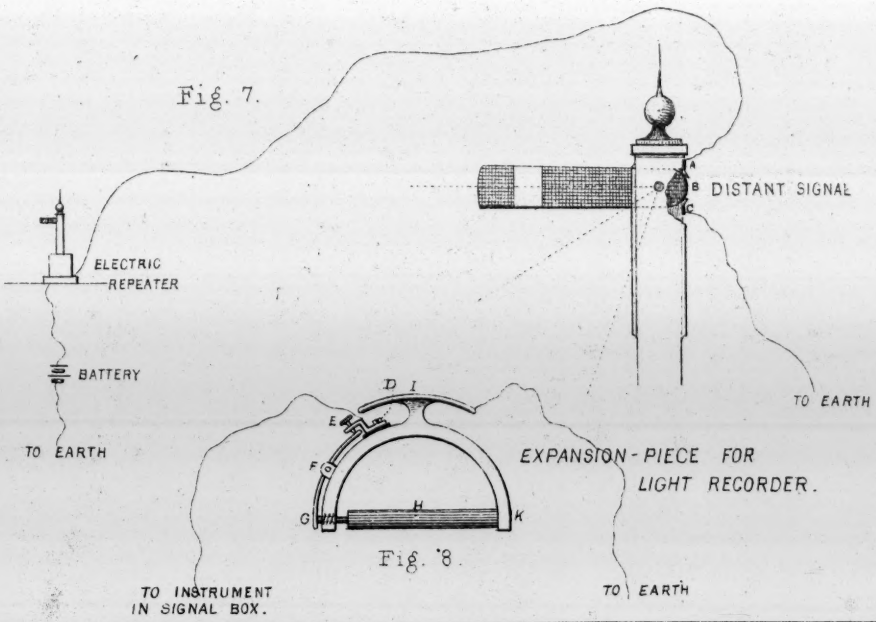


Fig. 8.

W. West & Co. lith.

Improved Railway signals.

Some of these may be further analysed as follows:

TABLE II.—ANALYSIS OF TABLE I.

Cause of Death.	No.
Mechanical Injuries—	
Fall from scaffold (ladder)	165
„ „ window	70
„ downstairs	456
„ in ships and boats	134
„ from height	500
„ in walking	93
„ (not stated how)	530
„ of heavy substances on	509
Horse or other animals	269
„ conveyance	1,250
Machinery	1,132
Fight	5
Blow, &c.	124
Gunshot wounds	185
Chemical Injuries—	
Burns	1,064
Scalds	701
„ (drinking hot water)	50
Lightning	21
Sunstroke	96
Exposure to cold	138
Asphyxia—	
Drowned	3,232
Suffocated by food	94
„ bedclothes	611
Hanged, strangled, and executed	581
Murder, manslaughter, and suicide	228

Now let us take accidents to railway passengers from causes within and beyond their own control:

TABLE III.—ACCIDENTS TO RAILWAY PASSENGERS, FROM CAUSES WITHIN AND BEYOND THEIR OWN CONTROL.

Date.	Within own control.	Beyond own control.	Total.
1871	45	12	57
1872	127	24	151
1873	120	40	160
1874	125	86	211
Average	104	41	145

This is an average of forty-one persons killed annually from causes beyond their own control; and it shows, in fact, that the railway companies are in reality more mindful of the lives of their passengers than the passengers are of their own lives.

These latter accidents can be classified as follows:

TABLE IV.—ACCIDENTS TO RAILWAY PASSENGERS IN 1874, FROM CAUSES WITHIN THEIR OWN CONTROL.

Cause of Accident.	No.
From falling between carriages and platforms	49
Getting out of or into trains in motion	22
Crossing the line at stations	33
Falling down stairs at stations	2
Falling out of carriages during travelling of trains	9
Other accidents	10
Total	125

This, however, is not the death-roll from all causes on all railways of the United Kingdom during the year 1874. The total number of persons recorded at the Board of Trade as having been killed was 1,424. Of these 211 were passengers, and, of the remainder, 788 were officers or servants of the railway companies, or of contractors, and 425 were trespassers, or suicides, or others who met with accidents at level crossings or from miscellaneous causes.

1874 was, however, a very exceptional year, for no less than 71 passengers were killed in the three fearful accidents on the Great Western at Shipton, on the Great Eastern at Thorpe, and on the North British at Bowness Junction.

We cannot contemplate these figures without feeling that, compared with other causes, the danger of railway travelling must be exaggerated. That there is danger in a train no one can deny. It is impossible to stand upon a platform when an express rushes by without feeling that there is but a rivet, a bolt, or a rod between life and death. Yet, though we have just read the harrowing accidents of a dreadful collision in the north, we unhesitatingly entrust our precious bodies in a railway carriage to the south, is a proof that we have faith in our railway management, and that we tacitly admit that there is also safety in a train. Our ideas of safety are but relative. If the press gave as much prominence to the 94 cases of choking, or to the 456 cases of falling down stairs, or to the 611 cases of being suffocated in bed, as it does to the railway accidents, we should hear less of the railway "juggernaut," and more of the skill and care with which the enormous railway traffic of the country is conducted.

The examination of the interesting statistics compiled by the Board of Trade shows that, taking the following periods, the proportion of passengers killed from causes beyond their own control to passenger journeys made was:

TABLE V.—PROPORTION OF PASSENGERS KILLED TO JOURNEYS MADE.

3 years ending	1849	. .	1 in	4,782,188	journeys made.
4 „	1859	. .	1 „	8,708,411	„
4 „	1869	. .	1 „	12,941,170	„
3 „	1873	. .	1 „	20,089,660	„

This gives striking evidence of steady and constant improvement in the conduct of the railway traffic, for the safety in the period ending 1873 was nearly five times greater than that in the period ending 1849.

If we take the average length of each journey at 10 miles, one passenger is killed, from causes beyond his own control, for every 200,896,000 miles travelled. Thus, if a person travelled 10 hours a day at the rate of 30 miles an hour for each of the 365 days of the year he would probably be killed in 1,835 years.

How has the potentiality of danger of railway travelling been converted into comparative actuality of safety? Freedom from accident depends upon the perfection of the road, of the rolling stock, of the signals, and, above all, of the men. But none of these elements are perfect. Accidents have been analysed into—

Defective permanent way	. .	18	per cent.
, rolling-stock	. . .	13	„
„ signals	. . . .	28	„
„ human machinery	. .	41	„

Classified for the period 1870–74, they show the result given in the table on next page.

Zeal and anxiety, the necessary evils of a state of tension due to increasing traffic; want of punctuality; late arrivals of the public; and variable weather, become an absolute source of danger. Every accident is traceable to its cause. Purely inexplicable accidents are unknown. Hence, though considerable improvements in the mode of working have been made—as are indicated in the continued progressive increase shown in ratio of killed to journeys made in Table V.—further improvements are certain. But all improvements bring their own evils, and the greatest of these is human fallibility. The body will tire, and the brain will get out of gear. Pure wilfulness, carelessness, or mischief are extremely rare. Who does not

Nature of Accident.	1870.	1871.	1872.	1873.	1874.
From engines or vehicles meeting with, or leaving the rails in consequence of, obstructions, or from defects in connection with the permanent way or works . .	9	19	21	24	18
From boiler explosions, failures of axles, wheels, tyres, or from other defects in the rolling stock	10	22	17	23	13
From trains entering stations at too great speed	—	2	7	5	—
From collisions between engines and trains following one another on the same line of rails, excepting at junctions, stations, or sidings	61	9	22	18	9
From collisions at junctions . . .	18	19	32	20	22
From collisions within fixed signals at stations or sidings, &c.	Included in the above 61.	63	91	98	75
From collisions between trains, &c., meeting in opposite directions	3	2	5	3	6
From collisions at level crossings of two railways	1	—	—	3	1
From passenger trains being wrongly run or turned into sidings, or otherwise through facing points	14	12	34	36	17
On inclines	6	11	9	11	7
Miscellaneous	9	12	8	6	—
	131	171	246	247	168

make a mistake? In the year 1874, 4,400,000 letters out of 967,000,000, or one in 220, found their way to the Returned Letter Office. 89,540 undelivered letters contained valuables, and bank-notes, bills, &c., the value of which alone amounted to 565,000*l.*; 337 of these had no addresses; 61,000 postage stamps were found loose in the different post-offices, and 20,000 letters were posted without any address at all.

How then is the comparative safety of railway travelling produced? By taking advantage of the lessons taught by experience, and by applying the means suggested by scientific thought and inventive skill to remedy defects. Failure has thus led to improvement. Every accident has been a lesson learnt, and bitterly have those suffered who have not profited by such writings on the wall. The particulars evidenced by each accident have been carefully and systematically recorded in the reports of the inspecting officers of the Board of Trade, and thus by recording past experience the materials are collected for carefully generalizing the laws of railway working and for establishing a true science of steam locomotion.

Telegraphy, or the art of conveying information by certain preconcerted signals to the ear and to the eye, is the chief aid

of the railway engineer. Thus, at every railway station, level crossing, or junction, signal-posts are erected which convey to the approaching engine-driver—by exposing discs, bars, or semaphore arms in different positions by day, or lamps displaying different colours by night—the fact that the line is clear for him to proceed or obstructed so that he must stop. The favourite signal by day—the survival of the fittest—is the arm, which, when at right angles, implies *danger*, and when at an angle of 45°, *safety*, and

> White means right: red means wrong:
> Green means slowly go along,

teaches the young railway lad the rule of the road by night. The character of every train is indicated by its *head lights*, and its presence to an approaching train by its *tail lamps*. Should thick weather prevent the sight of the signals, detonating fog signals announce the contiguity of danger. The marshalling of trains in station yards and platforms are produced by whistles and flags by day and lamps by night, all forming a species of telegraphic language between the fixed station and the moving train.

Where telegraphy is required to reach distances beyond the sphere of the ear or the eye, electricity is employed, and the electric telegraph becomes of prime and essential use, not only in regulating the traffic on double and single lines, but in securing safety. Special trains are moved about by its means, delays are remedied, breaksdown rendered harmless, runaway engines have been overtaken by its aid, passengers' luggage recovered; but, above all, irregularities are by its means rapidly announced, and the evils of unpunctuality rendered innocuous.

The greatest element of safety on railways is, however, the Block System.

The block system arose out of the multiplication of trains, and the necessity for increased speed. Necessity, the mother of invention, brought it into existence.

By it trains travelling upon the same line of rails are kept apart by a certain and invariable interval of *space*, instead of by an uncertain and variable interval of *time*.

The practice under the "time" system is to exhibit the danger signal for five minutes, and the caution signal for five minutes more, after a train or engine has been despatched from or past any station, junction, level crossing, or siding. Trains are thus said to be kept apart by fixed periods of five minutes, and if the caution signals were properly regarded, by an interval of time even longer than that. The safety of the train is entirely the responsibility of the driver. Immunity from accident is dependent upon his keeping a clear look-out. If engines ran

at regular and fixed speeds, if time-tables could be adhered to, if the line were not crowded with traffic, if the driver could always ensure a good view before him, if signals were near together and they were properly regarded, then a rigid interval of time might be maintained between following trains; but none of these elements of safety are constant. Fast expresses follow slow goods trains, now through a thick fog, now up a wet incline, at one moment in bright sunshine, at the next in a thick snowstorm; creeping mineral trains break down in a long interval between two stations; passengers rush in at the last minute, detain the train, and prevent the time-tables from being adhered to; trains are so frequent at some places that the five minutes' interval cannot be adhered to; obstructions to view arise from curves or cuttings, or from atmospheric causes; long lengths of line are unprotected by any signal at all, and signals themselves are too frequently neglected. Hence the system is brimfull of elements of danger, and the inexorable logic of facts has shown that the "time" interval is illusory and the system unsafe.

But when trains, however rapidly or slowly they may be running, however much punctuality has been infringed, however crowded with traffic the line may be, are invariably kept apart by an interval of one or two miles, collision between them becomes impossible. This is the *block system*, which has, very improperly, been divided into two classes, the *absolute* and the *permissive*. The former is the block system proper, the latter is not a "block" system at all, but a system introduced, not to secure the safety of trains, but to increase the capacity of the line for the transmission of increasing traffic. It is, doubtless, an improvement on the time system, but it bears little affinity to the block, and should certainly not be included in the same category.

The block system is effectually carried out by means of electricity.

The line is divided into sections, each having its own instruments for up and down trains respectively. Let A, B, C (fig. 1, Pl. CXXXIV.) represent three signal stations, and the space between them two sections of the line. Let the upper line be that for down trains, the lower that for up trains, and the centre the block signals. The connection between the latter exists only from one signal station to the other. Each line of rails has its own instrument, and each instrument its corresponding line, or out-door, signal. The block signals are for the guidance of the signalman; the out-door signals for the guidance of the engine-driver.

The electric signals consist of a miniature semaphore signal (fig. 2) capable of giving two signals—one, the arm raised to a

horizontal position, indicating *danger*, or stop; the other, the arm lowered or depressed, indicating *all clear*, go on—for each line of rails; a switch (fig. 3) by which the electric semaphore is worked, a bell (fig. 4), and a bell-key (fig. 5).

The semaphore is the *block* signal. The object of the bell is to indicate by varying numbers of beats the nature of the signal sent or about to be sent. Thus we have for the "warning signal," "train coming—look out," twice two beats—*i. e.* two beats followed by two beats, with a sufficient pause between to indicate it is not meant for four consecutive beats. Two beats for the "departure" signal, three beats for the "all clear" signal, six beats for the "obstruction" signal, and so on.

Let us now take diagram No. 1 and follow out the signalling of an up and down train. Say we have a train about to start from the terminal station A in the direction of B. It is a down train. The warning signal is first sent, and acknowledged by B. When the train is ready it is started, and its departure is notified. The warning signal was twice two beats sounded on B's bell; the departure signal two beats only. B now knows the train has left A, and it is his duty to prevent another following till the first has arrived at B. To do this he merely places his electric switch (fig. 3) at ON, which raises the electric arm for the down line at A to *danger*. The signalman at A has already, on the departure of the train, at the moment when he signalled its departure to B, placed his out-door signal for the down line at danger, and it has to be retained in that position till the electric arm is again depressed by B. No other train can now, without disregarding the signals, leave A for B. At this moment the position of the signals is that represented for down trains in the diagram (No. 1) between these two stations. The train is running through the section, and is protected in its rear at A.

B, on receiving the departure signal, has warned C that a train is coming by signalling to him the twice two beats upon his bell, in the same manner as did A to B. We will now assume that the train has reached B. It does its work at the station, if it is a station, sets down passengers, takes up others, and now it starts towards C, the electric arm for that section standing at "all clear." Its departure is announced, and C raises the electric arm at B against any following down train. The out-door signal for that line has been placed at danger, and now that B is perfectly clear of the train he is at liberty to tell A he can send on another train. He does so by placing his switch-handle (fig. 3) this time to OFF, which lowers the electric arm at B to the "all clear" position, and rings the bell three times. The signalman at A then lowers his out-door

signal to the same position, and all is ready for a train to follow.

Up trains are dealt with in a precisely similar manner. We have an up train running betwen C and B. At C we observe the electric signal for up trains shows danger, and the out-door signal the same; the signals in advance are at "all clear." When the train has arrived at B, done its work and departed, the section B C will be cleared, and C will be able to send on another train.

On single lines, that is lines which have only one set of rails on which both up and down trains are worked, additional precautions are taken. Before the train starts from any signal station the section is blocked against any train approaching in an opposite direction. It then leaves, and the station in advance of it blocks it in the rear. It is thus protected in advance and behind, section by section, throughout the line.

The operation of signalling, although somewhat tedious in description, occupies but a very little time; the pressure of a bell-key and the touch of the switch-handle are all that is required. The instruments are arranged in the most convenient manner over the signalman's frame, so that he can even carry on his electric signalling by one hand whilst with the other he operates the levers working the out-door signals. So rapidly is all this performed that trains frequently follow over busy sections of line within two and three minutes of each other. The sections are of course regulated to the traffic. Where it is dense they are necessarily short, the limit being the distance sufficient to enable a train to pull up before reaching the next signal station. Thus there are, in places, sections as short as half-a-mile, whilst in others they are five miles in extent.

But apart from the protection which electricity imparts to railway travelling, and the facility it offers for adjusting and regulating the traffic, there are innumerable purposes for which the telegraph is employed to facilitate business and to secure efficiency. The distribution of correct time, the collection of spare trucks and coaches, the relief of staff, the supply of assistance in cases of accident and danger, and—not least—the reparation of the errors and thoughtlessness of passengers.

It is used on some lines to establish an effective means of communication between passenger and guard; and perhaps one of its most useful applications is to record in the signal box, before the signalman's eyes, the position of the signal-arm by day and the condition of the light by night, which is hidden from his sight by the formation of the line, buildings, darkness, fog, or steam. Electric repeaters are one of the greatest elements of safety in working railways. Fig. 6 represents an arm and light repeater combined. A battery is placed in the

signal box (fig. 7), one pole of which is to earth, the other is connected to the instrument, from which a wire is carried to the signal post, where it is joined to a spring, A, in close proximity to the back of the arm to be repeated. To the back part of the arm itself is fixed a piece of metal, B, and in contact with it a spring, C. When the arm is raised *fully* to the *danger* position, the spring A is free from the plate B, but the slightest depression of the arm forms a connection between them, and so completes the electric circuit. The arm of the repeater is maintained at danger by gravity, and is lowered by the electric current. This arrangement can of course be reversed, *i.e.* the repeater arm may be raised by the current and lowered by gravity if required, or the movement of the arm in its several stages of danger, caution, and clear, can be recorded with the same wire.

The record of the light requires a separate and independent wire and battery. One pole of a battery is carried to the instrument, the other being put to earth, and a wire is then carried from the instrument to the insulated stud D (fig. 8) of an iron frame, I, fixed within the lamp at the signal post, in such a manner that the hollow tube, H, shall stand immediately over the flame. This tube, H, is firmly fixed to the iron frame at K, but is free to move at, and in the direction of, G. G F is a small metal lever centred at F, kept by means of a spring in contact with the adjusting screw, E. To the frame I is attached a wire connected with the earth. If, now, a flame be brought beneath the tube H, the heat from it will cause the metal to expand. In expanding, it will move in the direction of G, press against that end of the lever G F, and so carry its opposite extremity free of the adjusting and contact screw, E. In other words, it will break the electric circuit at E. The recording instrument is provided with an indicator having on it the words OUT and IN, which is so weighted, or adjusted, that when not under the influence of an electric current, the word IN shall be brought up to the aperture in front of the instrument, making it then read, as in the engraving, LIGHT IN. This, then, is what happens. The battery is always in circuit ready for action. So long as the light is burning, the tube H is expanded and breaks down the circuit. The moment the light goes out or gets dim, the tube again contracts, and the electric circuit being restored, the current flows through the instrument, carrying over the indicator so as to exhibit the word OUT instead of IN, and at the same time sets in motion a bell which continues to ring until the light has attention, or until the bell is turned out of circuit by means of the switch (ON) in front of the instrument.

The operation of scientific thought has introduced many

mechanical elements of safety into railway working, which are as ingenious as they are effective. Improved permanent way, the interlocking of signals and points; the concentration of levers in well-constructed cabins; effective brake power; perfect tyre fastenings; better coupling arrangements, and superior engine and rolling stock, have all aided to secure that simplicity of working and safety in travelling which undoubtedly exist.

But, as the principal element of danger in railway travelling consists in the fallibility of the human machine, it must not be forgotten that we owe our immunity from accident as much to the careful selection, education, and supervision of the staff and the maintenance of good discipline, as to the appliances of scientific skill. Science cannot be devoted to a nobler purpose than to the protection of human life, and the records of experience show that it has earned well-deserved laurels in rendering the dangers of railway travelling potential and its safety actual.

STUDIES OF MATTER AND LIFE.

By HENRY J. SLACK, F.G.S., Sec. R.M.S.

THE discoveries of recent science have greatly affected the notions we are able to form concerning the relations of force and matter, and likewise of the connection between physical agencies and manifestations of life. In studies of this description we are struck with the amount of force that lies potentially in extremely minute quantities of matter ready for vigorous action the moment the right stimulus is applied, and by the way in which quickness of motion makes up for smallness of weight. The physical inquirer is not obliged to tarry for the curious and important investigations of the metaphysician; he need not attempt to settle the fundamental questions—what is matter? and what is force?—in the ultimate constitution of either. To the experimentalist, matter is known by what it does; and whether the problem before him relate to mechanics, chemistry, electricity, light, heat, or gravitation, it is with matter in motion and exhibiting force, because in motion, that he has to deal. The same may be said of all the physical processes and manifestations of life, though we seem no nearer than the ancient Greeks were when we try to understand the connection between motions of particles and the phenomena of feeling and thought.

Light, heat, electricity in its various forms, chemical force and nerve force, are not only now classified as "modes of motion," but the motion in each manifestation of these forces appears to be wave motion; and it is probable that gravitation, the correlation of which with other forces is not yet established, may at last be found to be a wave motion also. In wave motion each particle moves, pendulum-wise, backwards and forwards in a small curve, transmitting the motion to the particles before it in more or less rapid succession, the motion becoming weaker as the original impulse is divided amongst more and more particles, until at last it is so enfeebled that it cannot be observed. The common illustration of throwing a

stone in a still pond and watching how, as the outward circling wavelets spread, they grow weaker and weaker, and if the space is large enough, at last seem lost in the calm beyond—this affords a good notion to begin with of wave forms and wave force; but suppose, instead of a stone striking the surface of water, a sudden explosion took place of a particle of dynamite at some depth below. Here we should have waves in all directions, ascending, descending, and spreading on every side. Such waves would bear somewhat the relation to the pond-waves that a well-known toy composed of balls within balls would do to a mere section of the whole concern. Wave beyond wave in consecutive series, spreading in all directions from a point, must be conceived as spherical shells one outside the other like the coats of an onion, each expanding and contracting within narrower limits, and sending the wave force and the wave form onwards to an indefinite extent.

The quantities of matter acted upon by these wave forces may be very small, and yet the power exerted very great. Thus "Faraday found the quantity of electricity disengaged by the decomposition of a single grain of water in a voltaic cell to be equal to that liberated in 800,000 discharges of the great Leyden battery of the Royal Institution. This, if concentrated in a single discharge, would be equal to a flash of lightning. He also estimated the quantity of electricity liberated by the chemical action of a single grain of water on four grains of zinc to be equal in quantity to that of a powerful thunderstorm."* Tyndall himself also beautifully illustrates this subject in his remark: "I have seen snow-flakes descending so softly as not to hurt the fragile spangles of which they were composed; yet to produce from aqueous vapour a quantity which a child could carry of that tender material, demands an exertion of energy competent to gather up the shattered blocks of the largest stone avalanche I have ever seen, and pitch them twice the height from which they fell."†

When galvanic electricity is employed to decompose water, the constituent elements, oxygen and hydrogen, are not merely *allowed* to separate, but they are pulled apart with great force, and Gassiot showed that if the water were confined in iron bottles an inch thick, a small battery gave force enough to split them asunder. The directive force which the particles of water obey in the act of freezing, and which leads to its expansion, has similar power; and, as is well known, a very small quantity of water will burst a strong shell or split a great rock.

Although heat sets the molecules of all sorts of matter in

* Tyndall, "Notes on Electricity," p. 15.

† "Heat as a Mode of Motion," 4th edit. p. 147.

motion, not purely wave motion, it comes to us in a wave motion from the sun, conveyed like light by a material so attenuated as almost to reach the supposed condition of spiritual existence. This ether of space, which we can neither see nor feel, approximates to the conception, if such can be formed, of an immaterial substance. It must be so thin, and so light, that an inconceivable quantity would be required to weigh a pound, and yet when in motion the marvellous speed of its oscillations enables it to exert gigantic force. It can act so mildly that we are utterly unconscious that any substance strikes our eye when we see, or have a sensation of, violet light, in consequence of 700 million millions of its minute waves dashing against it in a second. But light can also cause chlorine and hydrogen to rush together with enormous force, and it can instantly tear to pieces chemical compounds held together by forces equivalent to prodigious mechanical powers.

Professor Josiah Cooke reckons that if this ether (of whose existence he is not quite satisfied) were as dense as common air, it would resist pressure on each square inch of seventeen million million pounds, just as air balances one of about 15 lbs. without suffering compression. He also tells us that if we could confine ether in a cylindrical vessel of sufficient strength to bear the pressure, and put upon it "a cubic mile of granite rock, it would only condense the ether to about the same density as that of the atmosphere at the surface of the earth." *

In consequence of its wonderful elasticity, ether can convey light waves about a million times quicker than air can convey sound waves, and some of the pulsations that reach us from the sun, and which lie beyond the violet end of the visible spectrum, must make their extremely short wave oscillations much quicker than the 700 millions of millions of violet light. Should, contrary to probabilities, the theory that space is filled with ether, and that ether has the properties mentioned, be ultimately found untenable, the measures of wave lengths and wave velocities must still refer to something positively existing. Light comes to us from the sun at the rate of about 190† millions of miles in a second, whatever it is; and when physicists say a wave of red light is about $\frac{1}{39,000}$ of an inch long, and a violet one about $\frac{1}{57,500}$ long, no doubter of the existence of ether, like Professor Cooke, hesitates to assume that they are quantities of

* "The New Chemistry," p. 23.

† The exact distance can never be known, as some residual error is unavoidable. When all the Transit of Venus calculations are finished and compared with the experimental methods adopted in Paris, the average result will probably be not far from 190 millions.

something. The probabilities are, however, enormously in favour of the theory that ascribes certain properties to ether, and that light and heat consist in its undulations. All known facts coincide absolutely with this theory, and it has been the means of leading physicists and mathematicians to fresh discoveries.

What is this ether, that it possesses properties so extraordinary, and that, to speak in common language, a mere nothing of it in point of quantity can be the source or the vehicle of enormous powers? Professor Tyndall says it must be a material substance, but perhaps not a form of ordinary matter. If it is composed like common matter, its particles or molecules do not touch; and in that case it will be difficult to avoid the belief that there is a still more subtle kind of matter filling up the interspaces. If it be matter not divided into atoms or molecules, but continuous, we may expect to find that it will exhibit many properties and peculiarities not yet discovered, differentiating it from matter in ordinary forms.

Faraday followed Newton in feeling an invincible objection to the notion that matter could act through empty spaces, and, as we find in his life by Dr. Bence Jones, he was fond of quoting the following passage from a letter of Newton to Bentley:—"That gravity should be innate, inherent, and essential to matter, so that one body may act upon another at a distance through a *vacuum*, and without the mediation of anything else, by and through which this action and force may be conveyed from one to another is to me so great an absurdity, that I believe no man who has, in philosophical matters, a competent faculty of thinking, can ever fall into it. Gravity must be caused by an agent acting constantly according to certain laws; but whether this agent be material or immaterial, I have left to the consideration of my readers."

Faraday's own views on this subject were never very clear to other people. He recognised "lines of force," and spoke of "atoms" as centres of force, and not as so many little bodies surrounded by forces. The force was the atom extending indefinitely in all directions. According to these conceptions, "water is not two particles of oxygen (and hydrogen) side by side, but two spheres of power mutually penetrated, the centres even coinciding."* In the same place he said, "The force or forces constitute matter; there is no space between the particles distinct from the particles of matter."

We need not for present purposes pursue these speculations further. The wave forces mentioned communicate immense velocities to the molecules of matter, and these velocities are, in

* "Life of Faraday," vol. ii. p. 178.

fact, their powers. A gas—or common air, which is a mixture of gases—resists pressure and exerts pressure, because its particles are in vigorous, rapid, and ceaseless motion. Substances that are translucent, or transcalorescent, are so composed that ether waves go through them as water goes through a sieve. Bodies that do not allow light or heat to pass in this way, have their molecules set in motion by the impulse of the ether waves, and thus new forms of force are generated.

Tyndall's beautiful experiments on the powers of various substances to absorb heat and stop its radiation offer most instructive instances of the power exerted by small quantities of matter. Taking the absorption of one atmosphere of common air to be unity (1), it was found that this power was augmented thirty-fold when the same quantity of air was permeated by a little vapour of patchouli; lavender vapour raised it to 60 times, camomiles to 87, cassia to 109, and aniseed to 372. Upon these results Tyndall remarks "that the number of atoms in the tube (experimented with) must be regarded as almost infinite in comparison with those of the odours. . . . it would be idle to speculate on the quantities of matter implicated in these results. Probably they would have to be multiplied by millions to bring them up to the pressure of ordinary air. Thus—

> The sweet South
> That breathes upon a bank of violets,
> Stealing and giving odour,

owes its sweetness to an agent which, though almost infinitely attenuated, may be more potent as an intercepter of terrestrial radiation than the entire atmosphere from bank to sky." *

Wherever we find power exhibited, matter is in motion, and if the quantity of matter is infinitely small, and yet the power great, it is because the motion is infinitely quick. The waves of chemical force streaming from the sun are very short, and the quantity of matter acting in each oscillation, and tapping at the molecules on which it acts is inconceivably minute, but the taps are as inconceivably numerous and rapid. They are also rhythmical, and we know how the stone walls of a large building may be set in vibrating motion when an organ tone of the right pitch impels air waves to go on tap, tap, tapping till the whole fabric shakes.

We learn from these and similar facts that the wave forces can give great powers to infinitesimally small portions of matter, and that, as we are not able to place any limit that we can comprehend to the possible velocities of atoms and molecules,

* Tyndall, "Heat as a Mode of Motion."

so we are not able to assign any limit to the minuteness which would be incompatible with the exercise of effective force.

Becquerel has shown that when a membrane is moistened on each side by a different liquid, an electric wave force is set up, able to effect chemical decomposition. Thus the minutest part of the minutest gland, or of the smallest organism that is capable of assimilating external matter, is enabled to change the chemical condition, and pull asunder molecules or atoms that would resist the mechanical force of a steam-engine or a hydraulic press.

Unfortunately, we have no chance of seeing the ultimate atoms or molecules of matter. Chemists use the term molecule to denote the smallest quantity of any substance capable of existing alone; but the definition is not quite satisfactory, because they have reason to believe there are many compound molecules that only exist in parts of more complicated combinations. Could we, by help of any apparatus, see ultimate molecules, the sight would be an astounding one; for an extremely minute portion of any substance, however solid and quiet it might appear to ordinary vision, would be exhibited to us as composed of infinitely more particles than all the stars we can perceive in a clear sky, and all in motions as harmonious as those of the celestial bodies. When either compositions or decompositions are going on we should see hosts, by the myriad, rushing together, or springing apart, as the case might be. Eternal motion is the condition of life, whether of the smallest unit or of the entire universe. Nature, as Humboldt said, is ever arranging herself in new forms, and absolute stillness would be cessation of being.

The limits of visibility was one of the topics brought before the Royal Microscopical Society in February by the President, Mr. H. C. Sorby, in a remarkably able, and admirable, Annual Address.* Omitting the estimation of unavoidable errors in the construction of microscopical apparatus, and referring to researches by Pigott, Helmholtz, and Woodward, it seems that it is possible to distinguish the most favourable objects—alternate dark and bright lines such as in Nobert's test-plates—when they are as near each other as to be only $\frac{1}{112,000}$ of an inch apart, provided that several circumstances, which need not now be explained, are favourable. Minute spherules of about $\frac{1}{80,000}$ to $\frac{1}{100,000}$ of an inch may also be seen if their refractive power differs sufficiently from the fluid, or other medium in which they are immersed. It may, however, be affirmed that few objects less than $\frac{1}{80,000}$ of an inch in diameter can be seen;

* See "Monthly Microscopical Journal," for March 1876.

and of that size those only that are favourably placed. Mr. Sorby proceeded to inquire what sort of relation this power of microscopically assisted vision bears to the probable size of molecules of matter. He cited the results obtained by Stoney, Thompson, and Clerk-Maxwell, in attempts to calculate from different data the number of ultimate atoms in a given volume of any permanent and perfect gas at 0° C. and a pressure of one atmosphere. Thompson assigns as the greatest possible limit 98,320,000,000,000 in one-thousandth of an inch cube, which is $\frac{1}{1,000,000,000}$ of one cubic inch. Clerk-Maxwell, estimating the true number indicated by the phenomena of the interdiffusion of gases, made it 311,000,000; and Stoney, from his point of view, 1,901,000,000,000. The mean of these numbers is 50,260,000,000,000. In a letter received by the writer from Mr. Sorby, since the publication of his address in the "Monthly Microscopical Journal" for March, he assigns double weight to Clerk-Maxwell's calculations, for reasons that we need not stop to explain, and considers the number of atoms in a cubic $\frac{1}{1,000}$ of an inch of gas to be about 6,000,000,000,000, and that in the same space of liquid water the number of water atoms would be 3,700,000,000,000,000.

Water is essential to organic life: if an organism is thoroughly deprived of it, death ensues, though some creatures may be dried so as not to exhibit the least appearance of moisture, then pass into a dormant state, and become active again when more water and an appropriate temperature are supplied. The common rotifer and the *Anguillula tritici* have this property, and it is exhibited to some extent by that curious vertebrate, the Mud Fish, which survives an amount of drying that would be fatal to most animals as highly organised, though the baked mud in which it passes the hot dry season appears to prevent the desiccation from being carried too far for continuance of quiescent life.

If we say water is so valuable to organic creatures on account of its dissolving so many substances they need to be supplied with in a fluid state, we may be asked why water has such power, and it seems probable that they depend upon the immense number of its molecules, as well as upon their mode of aggregation. Each atom or molecule in motion tends to set adjacent atoms or molecules in similar motion; and a great number of small impulses, rhythmically repeated, easily set considerable masses of such bodies in fresh motions, differing more or less from those which belong to their own constitution. A child with a little hammer, tapping at a great log of wood, will in time set all the particles vibrating, and though each particle may move only through a small fraction of an

inch, when the whole log vibrates, the total quantity of motion is enormous, because the small motion of each particle is multiplied by millions and millions—that is, by all the particles the log contains.

Among the complex substances which chemists are acquainted with, no one could be named more important to organic life than albumen, which we all know in the condition of white of egg; and its remarkable powers of utility in the growth and development of plants and animals depends upon its extremely complicated structure. It contains a multitude of atoms of carbon, hydrogen, nitrogen, oxygen, and sulphur. It is usually found slightly alkaline; and some chemists, like Gerhardt, consider white of egg as a definite compound of an albumen acid with sodic hydrate, and believe other sorts of albumen have an analagous composition. Omitting, however, the alkali, Mr. Sorby takes as a probable composition of albumen C_{72}, H_{112}, N_{18}, SO_{22}; the letters representing the substance above named, and the figures the number of atoms which they contribute to the structure. With this view of albumen he finds that in a cubic $\frac{1}{1,000}$th of inch of horn there are about 71,000,000,000,000 molecules of albumen. A molecule of this substance, though much larger than one of water, is far removed by its minuteness from any possibility of human vision; and as Mr. Sorby explains in his paper, light is too coarse a medium to enable them to be seen, even if we could add sufficiently to the powers of our microscopes.

When so many atoms of various substances are built up together to form a new substance, there is reason to believe that they are arranged in groups, each group having a definite constitution, and being a distinct entity, at the same time that it has an appointed place and a definite relation to the whole. Each group may be regarded as a system in which the atoms composing it are in ceaseless motion, exerting force upon their neighbours, and keeping within certain bounds, just as the planets do that circle round the sun. Each group acts as a whole upon other groups, and thus there are motions of groups as well as motions of atoms, subject to the same conditions of keeping within bounds.

Now, it is evident that the wave forces of which we have spoken have great opportunities of effecting changes in such complex structures. One form or mode of wave motion may strike with its myriad pulsations at a group of atoms, another may strike at certain atoms in the group, and by such means some atoms or groups may be thrown out of their courses, and then the rest may form a new pattern, or, if suitable atoms of another sort are at hand, may take them in to what may be called their social system, and modify it accordingly.

The phenomena of the nourishment and growth of plants and animals depend upon actions of this sort brought about by the wave motions of heat, light, electricity, and so forth. Reproduction is, as Claude Bernard explains, intimately connected with nutrition. A particle capable of germination or growth receives an impulse from a particle of an opposite sex, that is, of one in a different molecular condition, and development is stimulated and caused to take place so as to repeat with minor variations the parent forms. The well-known facts of inheritance show that, although the female germ and the stimulating male element—the ovule and pollen grain of a plant, for example—are very minute, they are big enough to contain, in some form, or way, forces which cause all fresh matter that is assimilated to arrange itself so as to reproduce a series of parts repeating for generations with marvellous fidelity the parental types.

The same thing is noticed with animals in which the same species or the same race is reproduced from one generation to another with remarkable accuracy, extending to minute and often unexpected detail. For information on this subject the reader must be referred to the works of Darwin and other writers. What we have now to consider is whether the germ particles and sperm particles can possibly be conceived to contain enough molecules built up in definite patterns, so that, as Darwin in his theory of Pangenesis supposes, they can supply *parents* enough to enable us to regard each portion of a complex organism, plant or animal, as composed of their lineal descendants. "If," says Darwin, "one of the simplest Protozoa be formed, as appears under the microscope, of a small mass of homogeneous gelatinous matter, a minute atom thrown off from any part, and nourished under favourable circumstances, would naturally reproduce the whole; but if the upper and lower surfaces were to differ in texture from the central portion, then all three parts would have to throw off atoms, or gemmules, which, when aggregated by mutual affinity, would form either buds or the sexual elements. Precisely the same view may be extended to one of the higher animals, although in this case many thousands of gemmules must be thrown off from the various parts of the body." *

To compose a plant under this theory, the seed must contain gemmules which attract suitable matter to form root-fibres; other gemmules that in like way cause cells to grow and aggregate to make a fibrous stem, others to supply the sap, others to cause the growth and development of the leaves, flowers, and finally to supply the ovule and the pollen with a complete set of gemmules to carry on the process from one generation to

* "Animals and Plants under Domestication," vol. ii. chap. xxvii.

another; and as certain peculiarities of distant ancestors sometimes suddenly appear in their descendants, the ancestral gemmules must be sufficient in number to last for many generations, or they must act as parent cells and produce other cells.

Mr. Sorby applied himself to this problem, and sought to find what quantity of molecules existed in the quantities of matter that acts as germs and sperms. Supposing each gemmule contained a million molecules of the albuminous compound that is the physical basis of life, Mr. Sorby finds that "one thousand such gemmules massed together would form a sphere just distinctly visible with our highest and best magnifying powers." "If," he adds, "the gemmules were of much greater or much less magnitude, it appears to me very probable that Darwin's theory would break down from two opposite causes, or would need very considerable modification, because, if much greater, their number would be too few to transmit sufficiently varied characters, and if much less, they would scarcely contain enough of the ultimate atoms of matter to have a sufficiently varied individual character to transmit, since, of the assumed million ultimate molecules, only eighteen thousand would be of a true protoplasmic nature, the rest being water in molecular combination." Taking the $\frac{1}{6,000}$ of an inch as the mean diameter of a single mammalian spermatozoon, Mr. Sorby calculates it might contain $2\frac{1}{2}$ millions of such gemmules, and, "if one of them were lost, destroyed, or fully developed at the rate of one in each second, this number would be exhausted in about one month; but since a number of spermatozoa appears to be necessary to produce perfect fertilization, it is quite easy to understand that the number of gemmules introduced into the ovum may be so great that the influence of the male parent may be very marked, even after having been, as regards particular characters, apparently dormant for many years."

Again, taking the germinal vesicle of the mammalian ovum as $\frac{1}{1,000}$ of an inch in diameter, "it might contain 500 millions of gemmules," and "if these were lost or fully developed at the rate of one in each second, this number would not be exhausted until after a period of seventeen years." If the whole ovum, about $\frac{1}{150}$ in diameter, were all gemmules, the number would be sufficient to last, at this rate of one per second, for 5,600 years! This is, however, not probable; but Mr. Sorby's remarks have completely removed all doubts as to its physical possibility from the Darwinian theory, and they prompt us to a wonderful conception of the powers residing in minute quantities of matter.

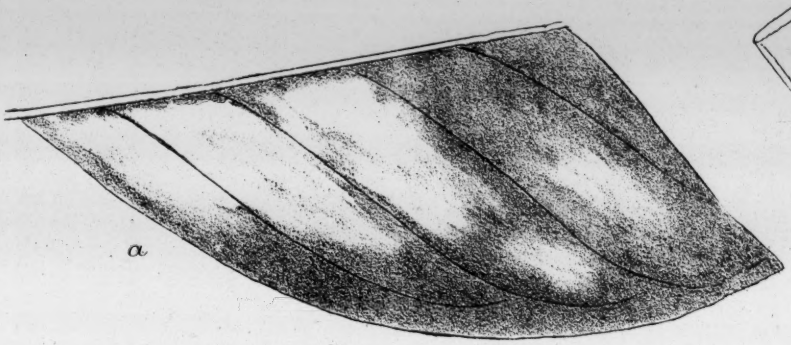
The student of nature stands surrounded on all sides by infinities. He can imagine no bounds to space or time; see no

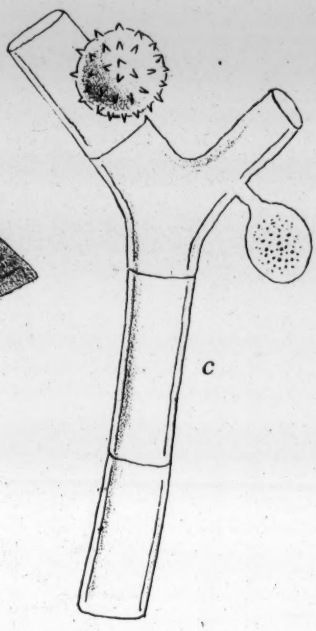
traces of a beginning, discover no symptoms of an end. There is an eternal flow and motion throughout the universe, a ceaseless change from power in the potential to power in the active form, and back again from the active to the potential—nothing added, nothing stationary, and nothing lost. Such is the aspect of the physical world, but what of the world of thought and will? Here we pause before a door of difficulty, and have no key to open. Let Du Bois-Reymond point it out:—

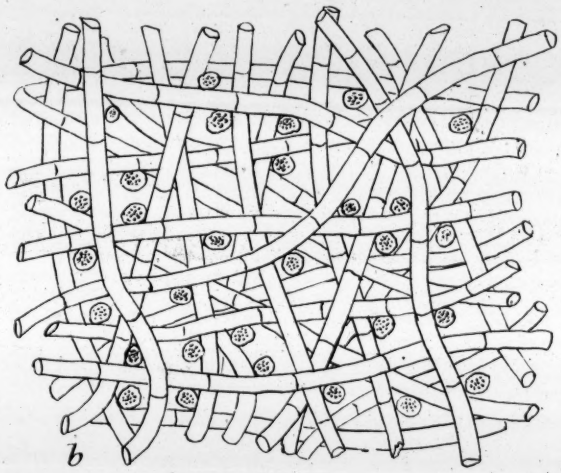
"Suppose we had arrived at an astronomical knowledge of the human brain, or even of an analogous organ in an inferior creature whose intellectual activity was limited to the sensations of well-being and discomfort. So far as regards the material phenomena of the brain our comprehension would be perfect, and our intellectual need to seek for causes would be satisfied in the same degree as it would be in regard to contraction and secretion, if we possessed astronomical knowledge of a muscle and a gland. The involuntary acts which emanate from nervous centres, without being necessarily connected with sensations, such as reflex and associated movements, respiration, tonicity, and lastly, the nutrition of the brain and spinal marrow, would be entirely known to us. It would be the same with the material changes that always coincide with intellectual phenomena, and which probably are conditions indispensable to them. And surely it would be a great triumph of science if we could affirm that such intellectual phenomenon was accompanied with certain movements of atoms in certain ganglionic cells and certain nerve tubes. What could be more interesting than to direct our intellectual vision inwards, and see the cerebral mechanism in motion corresponding with an operation of arithmetic, as we can watch that of a calculating machine; or to perceive what rhythmical movement of the atoms of carbon, hydrogen, nitrogen, oxygen, phosphorus, &c., corresponds with the pleasure we feel from musical harmony; what eddying currents of the like atoms attend the acme of delight, and what molecular tempests accompany the horrible suffering that ensues from irritation of the trigerminal nerve . . . ; but as regards the mental phenomena themselves, it is easy to see that, after acquiring an astronomical knowledge of the brain, they would remain just as incomprehensible as they are now. In spite of such knowledge, we should be arrested by those phenomena as things that are incommensurable. The most intimate knowledge of the brain to which we can aspire would only reveal to us matter in movement; but no arrangement, and no movement of material particles, can form a bridge to conduct us into the domain of intelligence. Motion can produce nothing but motion, or enter into the condition of potential energy. Potential energy in its

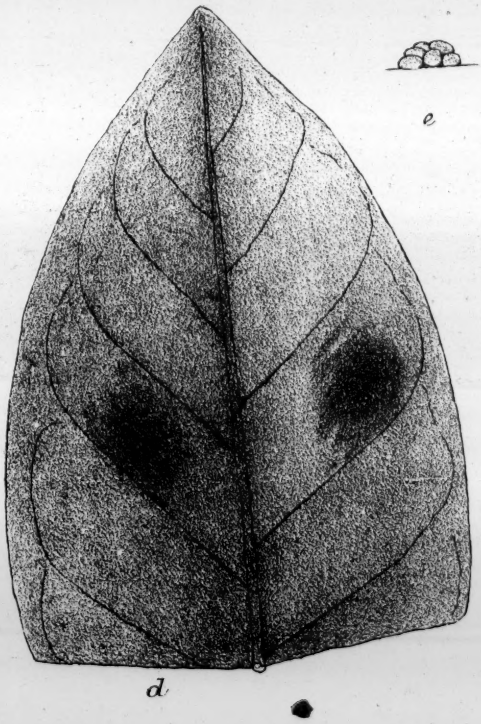
turn can produce nothing besides motion, the maintenance of an equilibrium, the exertion of pressure on traction. The total quantity of energy remains always the same. In the material world nothing can go beyond this law, and nothing can do less than it requires. The mechanical effect is precisely equal to the mechanical cause that exhausts itself in producing it. Thus the intellectual phenomena which flow from the brain beside of, but in addition to, the material changes that occur in it, are, to our intelligence, wanting in a sufficient reason. These phenomena remain outside the physical law of causality, and that is sufficient to render them incomprehensible." *

* Du Bois-Reymond, "Revue Scientifique," Oct. 10, 1874, p. 342.









M.C.C. del.

W. West & Co lith.

Coffee Leaf Diseases.

TWO COFFEE DISEASES.

By M. C. COOKE, M.A.

[PLATE CXXXV.]

THE cultivation of Coffee under no circumstances is such a profitable investment as to offer great inducements for speculation; but, unfortunately, during the past six or seven years, no less than three resolute and determined enemies to the coffee crop have successively made their appearance, and spread dismay amongst the coffee planters. One of these, the Coffee Borer, though by no means of the least importance, it is not our present intention to molest. The other two, being obscure members of the vegetable kingdom, alone come within the province of the botanist to describe.

Until within a few weeks it was not known in Europe, at least as far as we can learn, that more than one fungoid pest was making havoc amongst the coffee plantations in the East. Recently communications from planters in Mysore were sent to the Government of India, and these reports, together with specimens of infected leaves, have been transmitted to the India Office in London, undergone investigation, and resulted in a Report, which establishes, without doubt, the existence of a second fungal disease, which, in India at least, seems to be more destructive than the one previously known.

The "coffee-leaf disease," as it is usually termed, in distinction from the "coffee rot," is caused by a parasitic fungus. Of this there is not the slightest reason to doubt, although some have held to the opinion that it is the result, and not the cause, of the disease. There being still a few who hold similar views respecting the potato disease, it is by no means surprising that the same theory should be applied to the coffee plant. Its first appearance in the island of Ceylon was in the year 1869; and recent Reports from the planters in Mysore date its first appearance in Continental India in the same or following year. When suffering from this disease the leaves are marked by brownish somewhat rounded spots (Pl. CXXXV. fig. *d*), the under

surface of which is covered with minute orange dust, consisting of the spores of the fungus. These spores only appear on the under surface of the leaf, but the spots are observable also on the upper surface. The fungus is truly an endophyte living within the tissues of the plant, and expanding outwards. To its presence Dr. Thwaites attributes stains which are produced on the bark of the young branches, and the pale translucent spots which are to be seen on the leaves previous to the outbreak of the orange-coloured spores.

From the delicate mycelioid filaments which constitute the root-like portion of this parasitic fungus, threads arise in bundles which burst through the cuticle and produce at the tips of the threads the orange-coloured spores. Each of these spores has a somewhat irregular form, ovate, pear-shaped, or more or less of a kidney shape, covered externally when mature with minute globose warts, principally on the convex side, the shorter side being usually smooth (Pl. CXXXV. fig. *g*). When fresh they are of a bright yellow or orange colour, but this speedily disappears in drying. The longest diameter of the spore is from ·035 to ·04 of a millemètre, and that of the warts about ·003–·004 of a millemètre. We have observed these warts to leave the surface of the spores, and float in the medium in which the spores were examined as minute free globose bodies (Pl. CXXXV. fig. *h*). This was not noticed by Messrs. Berkeley and Broome in their examination of spores from Ceylon. Probably it may be dependent on the degree of maturity at which they had arrived. In the early stage the spores are smooth, and may be seen attached to the apices of the simple or branched threads on which they are produced (Pl. CXXXV. fig. *f*). When mature, the attachment is so slight that the least touch is sufficient to disengage them, and they appear to lie in little clusters on the discoloured spots, ready to be dispersed by the slightest movement of the leaves.

The fungus thus described was first determined and named by Messrs. Berkeley and Broome in the "Gardener's Chronicle,"* shortly after its first appearance in Ceylon, under the name of *Hemileia vastatrix*, and was regarded as a very interesting addition to science, from its forming a link between moulds and rusts.

The mode of germination has not been observed in this country. It is scarcely probable that vitality in the spores will extend beyond the loss of the yellow colouring of the endochrome. In Ceylon, however, Dr. Thwaites reports that it is not difficult to induce germination. Mature spores removed from a diseased leaf and laid upon charcoal kept continually moist, he says,

* "Gardener's Chronicle" for November 6th, 1869.

commence to germinate in a few days. "This process consists in the spore becoming somewhat enlarged, and its contents converted into one or more globose translucent masses. From each of these a filament is developed, which grows very rapidly, and becomes more or less branched. At the termination of some of these branches secondary spores are produced in the form of radiating necklace-shaped strings of little spherical bodies of uniform size, and this form closely resembles the fructification of an *Aspergillus*." Another observer in Ceylon (Mr. Abbay) has seen another form of secondary spores arranged in single rows of spherical bodies, a good deal larger than those radiately arranged, but still exceedingly minute. These inconceivably numerous secondary spores may be easily carried by the wind into surrounding districts, and thus convey infection to distant plantations. In what precise manner infection is accomplished is still unknown. The minute secondary spores may be absorbed by the rootlets, or they may enter by the stomata, or they may even germinate on the surface and insinuate their growing points through the natural orifices of the leaves. Neither is it yet known whether the successive attacks to which a plant is subject are all the result of one inoculation, or whether a fresh infection precedes each outburst of the disease.

In some features there is a similarity between this fungus and the red rust of cereals. Both of them burst through the cuticle and appear on the surface as an orange-coloured dust; in both the spores are at first produced on pedicels: but beyond this there is great divergence, especially in the mode of production of the secondary spores, of which there is no similar instance amongst the *Uredines*. The germination of a large number of species amongst the *Uredines* has been observed, but hitherto in no instance have minute secondary spores, growing in chains, been recognised. There is doubtless another feature in which the coffee rust resembles the wheat rust: it is not likely to yield to the application of sulphur, as the white moulds usually do. Sometime since the application of a diluted Condy's fluid was recommended as an efficient check for the Hollyhock disease, and it is not improbable that what would succeed with the *Puccinia* would be beneficial in the case of the *Hemileia*; but hitherto it has been affirmed that nothing has proved effectual in checking the ravages of the coffee disease in Ceylon. Many of the coffee planters in Mysore declare that this disease in their plantations has not sensibly affected the quality or quantity of the crops; in others the contrary is affirmed.

The second, and more recently known, coffee disease has not at present been recognised in Ceylon, although Indian planters declare that it certainly has been present on coffee estates in

Mysore almost as long as the "coffee-leaf disease." Mr. G. Porter says that "it is very prevalent in the Mulnad portions (and more especially in the Kadur district) of the Mysore country. It is known as the 'kole roga,' or 'black rot,' and not only does it attack coffee, but it does great havoc in the betel-nut gardens. He was told by the natives that they had hardly ever known a wet season without it, some years worse than others. It is this disease that coffee suffers far more from than from that of the *Hemileia vastatrix*. It makes its appearance about July, when the leaves of the trees affected by it get covered with a slimy gelatinous matter, and turning black drop off; the berries likewise rot and fall in clusters. He estimates that the planters lose nearly one quarter of their crop each year by this plague. Gangs were sent round last monsoon to collect the diseased leaves, which were carried off the estate and burnt; and although this did some good, it was not the means of abating it to any extent."

The leaves affected with this "rot" are spotted on the under surface with large greyish-white irregular patches, sometimes occupying nearly the whole surface, sometimes in spots limited by the larger veins (Pl. CXXXV. fig. *a*). These patches are quite smooth to the naked eye, with all the appearance of a superficial incrustation. When moistened, the entire spot can be removed with the point of a lancet, by stripping it in a delicate hyaline film, somewhat like a film of gold-beater's skin, showing no attachment to the leaf, unless of a very slender and superficial nature; indeed, the film may be rolled up under the thumb and finger. Under the microscope this film is found to consist of a closely interwoven web of hyaline septate filaments, often branched, and crossing in all directions (Pl. CXXXV. fig. *b*). They are usually from ·005 to ·0075 of a millemètre in diameter. On the upper surface these threads are studded at irregular intervals with small globose echinulate spores, which are seated upon the threads, without any visible pedicel, although when first formed there appears to be a short stem, which is ultimately absorbed (Pl. CXXXV. fig. *c*). These spores are about equal in diameter to the diameter of the threads. The threads and spores seem to be agglutinated together into a film by some gelatinous medium, so that not a spore or thread can be removed from the mass without difficulty. In this feature the "rot" differs from nearly all the *Mucedines*, in which the spores are so slightly attached that they float away on the application of moisture, whilst in the present instance no application of fluid avails to disturb a single spore.

In order to examine the fungus in as complete a manner as possible, a portion of the leaf is immersed for twelve hours in water, but this does not dissolve the mucus so as to free the

spores. Whether examined in water, spirit, or glycerine, the results are the same; but in nitric acid the threads are at first more distinct, but gradually become absorbed into an indistinct mass. When the film is stained with aniline or roseine, the threads and spores are brightly coloured by the medium, so that the details may be better observed. There is, however, still considerable difficulty in penetrating the film with a high power, and the threads will not separate.

From an examination of this fungus, with a view to the determination of its scientific relationship, we have come to the conclusion that it has no very close affinities, that it is not only specifically new, but will have to be accepted as the type of a new genus.* Whether it is in itself an autonomous species, or a condition of some other and higher form, cannot be determined from present information; at any rate, it is so far complete as to possess a vegetative and reproductive system. The globose echinulate bodies have all the characteristics of spores, but more than this cannot be affirmed until some one is fortunate enough to observe their germination, or all endeavours to do so should fail.

The principal scientific question which presents itself in relation to this fungus is its relationship and affinity. Two or three suggestions have already been offered on the subject; although made without any microscopical examination of the plant itself, they are worthy of a passing notice. One suggestion is that the supposed fungus may be an imperfect condition of some lichen. It may be true that low forms, or imperfect states, of lichens are sometimes found on the living leaves of growing plants, yet the structure is hardly such as those lichenoid bodies assume. Considerable emphasis is sometimes placed on the presence of gonidia in the lichen thallus as distinguishing it from fungi. There is no manifestation of such bodies in the present instance, and it would be more satisfactory for such an objection if a similar authentic instance could be adduced of a destructive leaf-parasite which is an undoubted lichen. Another suggestion has been offered that it may be a low form of Hymenomycetous fungi. If so, it should at least give some indication of its relationship. As spores are undoubtedly present, there should also be basidia, bearing these spores in pairs or quaternate; at least, there should be some evidence of an approach to such low hymenomycetal forms as *Exobasidium* or *Hymenula*. Probably it was some such organism as *Exobasidium* which was thought of when this suggestion was made, but, certainly, we can observe no relationship whatever between them.

* "*Pellicularia Koleroga*"—Cooke in Grevillea, iv. p. 116. "On the Affinities of *Pellicularia*," in Grevillea, iv. p. 134. "Report on Diseased Leaves of Coffee and other Plants."

Apropos of the suggestion which has been offered through the medium of a horticultural newspaper, that the "black rot" appears to be the *mycelium* of some fungus, it will be sufficient to remark that the term "mycelium" is, by general consent, confined to productions which consist of barren threads. The presence of spores, in this instance, clearly removes the production beyond the limits of the term "mycelium." Unless terms are employed with their recognized meaning and limitations, some explanation should accompany their use to prevent misconception.

The conclusion at which we have arrived appears to us the most tenable one, that the fungus in question belongs to the *Hyphomycetes*, or moulds. In habit and external appearance it strongly reminds one of the white mould which precedes many species of *Erysiphe*, such as the one so common on peas in the autumn, or that which precedes *Uncinula* on the leaves of the maple. Even under the microscope there seems to be some kind of relationship; the interwoven, septate, colourless branched threads are present, but there is an addition of a somewhat gelatinous medium, which binds the threads together into a pellicle. The spores and their mode of production are different, and this, in the *Hyphomycetes*, is a most important distinction. In *Oïdium* the spores are produced in chains, in the present species singly. It is very true that the structure, as seen in a drawing, resembles closely that of some species of *Zygodesmus*, but there is a peculiarity in the threads of many of the species in that genus that the threads are cut, as it were, nearly through at short distances, or abruptly bent, of which there is not the slightest indication here. The spores are very similar in size and form, but there are two or three features which appear to us conclusive for rejecting the coffee rot from this genus. In all the species of *Zygodesmus* the threads are free from any investing medium, the spores are pulverulent, and, moreover, the threads are more or less coloured. Further than this, all the species occur on dead wood or leaves, and in no instance is a species parasitic on living leaves. Although too much reliance is not to be placed on this fact, it is nevertheless noteworthy that in genera in which the species are parasitic on living plants there is seldom an exception to this rule, and so in genera which contain species found on dead substances parasitic species are not found. In illustration of the former we may cite *Peronospora*, *Ramularia*, and *Erysiphe*, and of the latter *Dactylium*, *Sporotrichum*, and *Zygodesmus*.

The presence of the gelatinous element which binds together the threads and spores into a thin pellicle, which is easily separable from the matrix when moist, is an important feature in determining the affinities of the "coffee rot." In the genus

Amphiblistrum of Corda there is said to be such a gelatinous medium. In many species of *Fusisporium* there is something of the same kind; in *Alytosporium* as constituted by Link, and in some other genera allied to *Sporotrichum*. Still, from all these there are such manifest points of divergence that no one would venture to associate the present species with any of them. Hence no other course appeared to be open to us but to constitute *Pellicularia Koleroga* the type of a new genus, allied to those just alluded to, but distinguished therefrom by its parasitic habit, sessile, echinulate, globose spores, and the freedom with which it separates from the matrix. Whether or not mycologists will accept this as a sufficient distinction, the present course has not been adopted without much consideration.

The fact of an epiphytal fungus, which does not penetrate the tissues of the leaf, being so destructive to the foster plant, may at first seem strange, until it is remembered that in plants with coriaceous leaves all, or nearly all, the stomata are confined to the under surface of the leaf. If, therefore, a filmy substance like the present fungus overspreads the under surface of the leaf, and securely seals up all the stomata, it is but reasonable to expect, not only that the leaves should fall, but that the plants should suffer injury. In such diseases as that which affects the hop, and which is but too well known to hop-growers in this country, the chief destructive action lies in the closing up of the orifices of the leaf by the woolly coating of mycelium produced by the fungus.

From the similarity of habit and growth in the coffee rot to that of the hop mould, and, we may also add, of the vine mildew, it is extremely probable that the remedies which have been applied in the latter instances with success would be more or less advantageous in the former. It is now generally admitted that the application of the flowers of sulphur, by dusting over the leaves, is the most effectual remedy yet discovered for hop mould and vine mildew. It should certainly have a fair trial also in the case of the "coffee rot," and the presumption is strongly in its favour.

We have stated sufficient to show that the coffee plant in Mysore and in Ceylon is exposed to considerable danger from the two persistent enemies above described. Some plants of the Liberian variety were sent out some time since in the hope that its apparently stronger constitution would prove impregnable to the *Hemileia*. At first they progressed favourably, but subsequent information dispelled the hope, for the coffee disease had attacked them also. Unless some remedy is discovered there is reason to fear that even if the cultivation of coffee is continued in these places it will be unremunerative, or,

the costs and risks of the venture being so much increased, a corresponding increase in price will be a natural result. We have the experience of the vine and potato diseases, and more recently of the hollyhock disease, to warn us how surely and speedily the area of infection is extended, and though for the present some coffee-producing countries are free, one or other of the pests may at any time make its appearance where it has never been known before.

DESCRIPTION OF PLATE CXXXV.

a. Portion of leaf affected with "black rot."

b. Fragment of pellicle formed by the fungus *Pellicularia koleroga* × 500 diam., showing threads and spores.

c. Small portion of thread further magnified, with young and mature spore.

d. Portion of leaf affected with *Hemileia vastatrix.*

e. Tufts of spores as they appear clustered on the spots, slightly magnified.

f. Young threads and spores × 500 diam.

g. Mature spores of *Hemileia vastatrix* × 500 diam.

h. Globose bodies, or warts, from the surface of mature spores of *Hemileia* × 500 diam.

AMONG GLACIERS RECENT AND EXTINCT.

By the Rev. W. S. SYMONDS, F.G.S.

IF we would see *King Frost* on his throne, we must travel to the Arctic or Antarctic regions. Of the latter little is known, save that there are mountains rising to the height of 15,000 feet, that the Antarctic continent is at present ice-bound and going through a glacial epoch, and that ships are stopped by pack-ice before reaching the 70th degree of latitude. Victoria Land, which extends from 71° to 79° south latitude, was ascertained by the exploring expedition of Sir James Ross (1841) to be fringed by an enormous barrier of ice; while the inland country rises from 4,000 to 15,000 feet above the sea, as in Mount Melbourne, and the crater of Mount Erebus is elevated to the height of 12,000 feet. Graham's and Enderby's Lands, in the Antarctic regions (lat. 64° and 68° S.), are situated in the same parallels of latitude as are those regions of the northern hemisphere which are inhabited by man and herds of wild animals; but the Antarctic lands are not known to possess a single land animal, and are wild, wintry, and desolate in the extreme. Frost reigns everywhere! In the Arctic regions of the distant North voyagers have explored much the seas and border lands—the home of Arctic men, and the abode of the musk ox, the polar bear, the walrus, and the reindeer.

The accounts of Greenland are remarkable. Some travellers who have penetrated a few miles into the interior of southern Greenland describe it as occupied by one vast glacier, and state that in 70° N. the land of the interior is covered by one vast ice-sheet of unknown depth, which conceals and obliterates all indications of hill and valley. This vast mass of inland ice is in constant motion, creeping and advancing slowly, but with different velocity, in different places towards the sea. Near the sea it presents "ice-walls" rising sometimes to the height of 3,000 feet, and from these break off the iceberg and icefloe, with a crashing and then a roar like the discharge of a park of artillery. Some of the bergs ground in the fiords and break up slowly, others sail off to the ocean, sometimes rising to the

height of 200 feet, looking like vast glassy towers and minarets as they move steadily out to sea. We learn also from the recent expedition of the German Expedition of the *Germania* and *Hansa*, that the north-eastern district of Greenland is a great contrast, as regards climate, glaciers, and animal life, when compared with southern Greenland. In 75° 29′ of latitude N. they found abundance of reindeer. In 73° 13′ N. a large fiord was discovered and entered by the *Germania*, and she ascended it for seventy-two miles. The further they ascended the warmer became the temperature of the air and the sea. The scenery of the country of South Greenland, about 70° N., is described by Mr. Whymper as "completely covered with glacial ice up to a great altitude, and at the summit formed a dead level." The scenery as described from the reports of Captain Koldeway and Dr. Laube, up this north-eastern fiord entered by the *Germania*, is "Alpine," "beautiful," and "imposing." "An unknown land—the real interior of Greenland—revealed itself before their astonished gaze." Some mountains were ascertained to be as high as the Matterhorn (14,000 ft.), and one 7,000 feet high was ascended. Numerous glaciers, waterfalls, and cascades came down from the mountains, and reindeer and musk oxen roamed about in herds. Ermines and lemmings were also met with, but the Esquimaux appear to exist no longer in this district.

The experiences of the crew of the *Hansa* were very different. She was driven far to the south by winds and currents. *Frost* caught her by the middle of Sept., and the ship was frozen hard and fast, without hope of escape from a winter in the ice. In Oct. the ice crushed up the ship, and the *Hansa* sank, leaving the crew on an icefloe. On this icefloe for 200 days they lived and drifted, struggling continually amidst ice and storms, and suffering frequently from bitter hunger. In May they were enabled to launch their boats, and on June 13 reached the station of Friedrichstal, after enduring the most terrible hardships.* We learn from such voyages the importance the glacialist must attach to the drifting of icefloes and icebergs. The superficial area of the floe on which the crew of the *Hansa* were saved was upwards of three square miles; and arctic voyagers, in the high northern latitude of Spitzbergen, have seen bergs drifting along loaded with thousands of tons of rocks and earth, which on melting fall to the bottom of the sea, or against some coast or island near which the berg is stranded. Such must have been the history of the transportation of the boulders which have come from Norway and Sweden, and which were deposited on the coast of Norfolk, above the submerged Cromer

* See "Proceedings of the Royal Geographical Society," vol. xv. No. 2, July 1871, p. 102, &c.

forest bed and the skeletons of its extinct mammalia. Seeds and berries of herbs, and shrubs too, must be often carried on ice-rafts out to sea; and we may owe our Arctic plants which still linger on our mountain summits in Great Britain to such migrations in glacial times. Bears and wolves have often been seen and heard on icefloes, hundreds of miles from the shore; and the crew of the *Hansa* are not the first men who voyaged upon a floe.

I shall allude to Greenland again as revealing another history. It is the lot of few men to behold such ice and frost phenomena as I have alluded to. Ordinary mortals must be content with ascending mountain heights in more temperate latitudes, where the atmosphere becomes colder as we ascend; for even in tropical regions there are mountain heights where frost does its work. In the Swiss Alps the snow line is about 8,500 feet above the sea, so that in the lofty recesses of the high Alps, which reach from 12,000 to 15,000 feet in height, the moment the sun goes down, frost sets in, even in the summer time, and the icicle droops from the rock, at night, where, in the daytime, dripped the runlet. In Switzerland, as is well known, we may ascend to regions on the summer snow-line, where comfortable hostels are established, and we may investigate snow and ice and glaciers. Such is the Inn of the Riffel, above Zermatt and the great Gorner Glacier, and from near which we visit the Monte Rosa glaciers, and behold a panorama of snow mountains forty miles in diameter. Such snow mountains are the great feeders of the glaciers of the Alps. Everyone knows what a glacier is, and how they are rivers of ice, frozen masses, which descend sometimes for a distance of twenty miles from Alpine heights to the valleys below. We need not enter into the disputed cause of their *motion*. I propose rather to direct attention to phenomena presented by these products of frost which come within the observation of all travellers in Alpine regions, and the observation of which greatly enhances the enjoyment of Alpine travel. Unlike the continental ice of South Greenland, above which no rock rises above the ice to become shattered by the frost, or weathered by the gale, to send down its débris upon its glassy bosom, in Alpine districts the mountains rise sometimes thousands of feet above these rivers of ice, as they creep down the valleys and gorges; and great masses of rocky débris fall on them, the result of the continuous and alternate action of thaw and frost. These accumulations are termed *moraines*: and there are lateral moraines, which are piled on the sides of a glacier; medial moraines, which are formed when two streams of ice meet; and the terminal moraine, where the débris is deposited at the termination of the ice.

Now with regard to *moraines*, I advise every traveller among glaciers who would add to the enjoyment of his tour to learn

as much as possible, by the aid of geological maps, the evidence of a trusty guide, and above all by personal observation, what the rocks consist of which overhang a glacier at different localities along its course. For example, the great Aletsch glacier is more than twenty miles in length, and in its moraines are rocks of totally different formations: some are wanderers from the source of the glacier among the great snow-fields; others are derived from rocks *in situ* nearer to the foot of the glacier at the Bel Alp. Again, at the Zmutt Glacier near Zermatt, a rock fragment which has fallen from the Matterhorn is a volcanic rock, and differs in toto from a fragment from the Dent Blanche. In many instances, and many localities, these ice-borne masses render evidence that the ice of existing glaciers once rose far above their present surface, and, stranded high upon mountain flanks, sometimes a thousand feet above the glacier, or the site of a glacier long since melted away, it is often of importance to the geologist to know the home of the parent rock from which this or that wanderer has been derived. This is especially the case in hilly countries, where much débris has fallen from the rocks into the mountain vales. More than once it has occurred to me to be sent to see old glacier moraines which were no moraines at all, and which would not have happened had my friend known the difference between atmospheric débris and the erratic masses which more or less are generally to be traced in true moraines. It is well also to accustom the eye to recognise another witness of the existence of a glacier—viz. the "*roche moutonée*," so called from the supposed similarity to the round and smooth outline of a sheep's back when newly sheared and when lying down. These *roches moutonées* are portions of the rock, *in situ*, over which a glacier has passed, and owe their round and smooth outline to the action of the ice continually grinding over them and planing off the inequalities. *Blocs perchés* also are rock masses which have been carried upon a glacier often for long distances, and left perched, on the melting and retiring of the ice, far from the parent rocks from which they were derived. Among the Mont Blanc Alps are rocks of different character. There are granite rocks, gneiss rocks, conglomerates and various others; and it is very striking to see a *bloc perché* of Mont Blanc granite, glistening white in the sun, perched upon some dark rock on the side of a hill, far from the noble mountains from which it has travelled.

Some years have passed away now since I accompanied my friend Sir William Guise, who also loves the lore of the rocks, to examine glacial phenomena among some of the noblest of Alpine scenery, to collect Alpine minerals and fossils, Alpine plants, Alpine butterflies, and Alpine everything. We first visited the

glaciers of the Rhone and Viesch, and the great Aletsch glacier, and later on we went to the Monte Rosa district and its glaciers. At the foot of the Rhone glacier we saw proofs of the recent gradual diminution of the ice in that district, when we examined the terminal moraines which are arranged concentrically one within another, showing the retiring, step by step, of the ice foot. We soon found that the glaciers of the Alps are not everywhere shrinking at the present time. The Gorner and Findelen glaciers, which descend from the Monte Rosa snow-fields, are *increasing*, for we found the great Gorner glacier towards the base ploughing up the green turf and cherry-trees thereon. Men too are still living who can remember encroachments on considerable tracts of land, and even that the Gorner glacier thrust boulders through the walls of chalets, some fifty or sixty of which were destroyed by the protrusion of the ice. The Zmutt glacier, too, which takes its name from the vast masses of rock which cover its lower extremity, should be visited to see what an amount of moraine matter is travelling slowly onwards towards the rushing river which flows in a torrent from its icy interior. Above it rises on one side the magnificent Matterhorn, which sends down masses of greenstone to the glacier, to travel onwards with the gneiss of the Dent Blanche and Col d'Erin. Our guide over this glacier was one of those who the year before went to assist in bringing in the mangled remains of Mr. Hudson, Mr. Hadow, and Michael Croz, after that fearful fall from a precipice of 4,000 feet, on the side of the Matterhorn above the glacier. The body of Lord Francis Douglas they never found, and it was supposed to have fallen into a crevasse. I need hardly allude to the grand scenery around Zermatt and the Riffel, with Monte Rosa and her glaciers, the Lyskamn, the Breithorn, the Matterhorn, the Dent Blanche, and the Weishorn as seen from the Gorner Grat and the Cima di Jazi, which itself is a scene of enchantment as you look upon these magnificent mountains on whose summits snow and frost reign supreme, and which are seldom trodden save by the chamois, or floated over save by the lammergeyer. Here, too, you stand face to face with the tremendous eastern precipice of Monte Rosa, while 6,000 feet below lies the Macugnaga glacier, and away, far below that, the green vineyards of sunny Italy. Yet even here, though surrounded with snow mountains, from which flow great rivers of ice, around the Riffel, on the flanks of Monte Rosa, or at the Gorner Grat, every place that is free from snow reveals evidence of the once greater extension of far larger glaciers in roches moutonées, blocs perchés, and ice action of other days. But before alluding to the phenomena of *extinct* glacial action more fully, allow me to say that I know of no glaciers where the results of ice action, or glaciation, can be studied in greater

perfection than among the Mont Blanc Alps around Chamounix. I visited this district during the autumn of 1875. Here the changes are most marked within the last thirty years. Sir Wm. Guise pointed out to me the spot where the foot of the glacier of the Mer de Glace then extended beyond the Arveiron's source. It has retreated nearly a hundred yards. Higher up, near the Montanvert, the glacier has sunk twenty feet below the rock from which he, thirty years ago, stepped upon the ice. Such has been the gradual diminution within that period. The older guides confirm Sir William's statement, and the correctness of his observations. Here all around we may study moraines far from the glacier, roches moutonées of gneiss, worn and polished as if the ice had vanished suddenly the day before, and bare and brown as if scathed by fire; with here and there a bloc perché of white granite which has travelled from Mont Blanc.

Near the source of the Arveiron the Mer de Glace has piled up a great terminal moraine, rising into a long low hill, one side of which is covered with trees. This hill is made up of masses of rock of all sizes, which once were wanderers upon the slow moving glacier when it thrust its ice foot beyond the present termination. One great erratic of Mont Blanc granite has been marked with a large red 2, by the Geological Society of Switzerland, to prevent its being blasted by gunpowder for building purposes. This erratic is as large as a good-sized cottage, and is an example of the size of the rock masses which now and then fall from the hills above the glacier, and are carried forward often for many miles upon the ice. Numerous bloc perchés are stranded on the flanks of the hills on the right bank of the Mer de Glace, as you descend from the Jardin to the Chapeau, which the observer may at once see do not belong to the rocks on which they lie stranded. There are some rock masses, too, which have been derived from different localities, now perched on the ice towards the foot of the glacier where it comes down into the valley among chalets, and pine woods, and Alpine gardens.

Such are some of the modern phenomena the geologist learns to observe among glaciers and ice action. These, however, are merely subsidiary to the interest with which we pursue the examination of the evidence that there were, in former days, glaciers of *colossal dimensions*, which filled the great vales of Switzerland with ice, and which reached from the Alps to the Jura, across where is now the Lake of Geneva. To this strange history belongs the transportation of the celebrated Mont Blanc granite boulders, which are stranded 900 feet above the Lake of Neufchâtel, and of which the Pierra à Bot is the most remarkable. Again, on the east of the Lake of Geneva, opposite Bex, are the great blocs perchés of Monthey, which lie

stranded against the hills that bound the Rhone valley. Most of them are of granite, and, if not quarried for building purposes, there was one which was calculated to contain some 20,000 tons of granite. They lie perched several hundred feet above the Rhone, and are the relics of a great glacier that once filled the valley, during the period known to geologists as the *Glacial Epoch*. Nor is the evidence of the former great extension of glaciers deficient near Chamounix for those who look out for such proofs. As already observed, high above the Mer de Glace may be seen blocs perché, moutonéed rocks, and moraine matter piled *high above the present glacier*. Again, as we journey from Chamounix to Martigny up a beautiful valley skirted by pine woods, we see the picturesque village of Argentiere opposite the Silver glacier, which descends to the valley from the magnificent Aiguille which towers above. Beyond this glacier is a great terminal moraine, forming a wooded barrier right across the valley, save on the left bank, where it is cut across by the river. This terminal moraine must have been piled up by a great glacier which once came down the valley from the Col de Balme, for it is made up chiefly of erratics derived from that district. The ice is gone, and in the place thereof we have the pine woods, the village, and the rushing river, with marks of glaciation everywhere on the sides of the valley. Beyond Argentiere, too, up the valley of Berard, on the left hand in going to the Tête Noire, are some large blocs perchés ranged in lines at a height of more than a thousand feet above the valley. These are erratics which were stranded where they now rest by a stupendous glacier which swept down from the mountains from the direction of Trient, and the mountains above the Valorcine, and of which now not a fragment of ice is left. But we will carry our investigations still farther. At the lower end of the great Italian lakes, such as Lago Maggiore, Como, Garda, and others, there are immense moraines which have been brought by extinct glaciers from the upper Alpine valleys far above the lakes. Professor Ramsay suggested that the origin of all these lakes is owing to the crushing and grinding action of enormous glaciers descending from the high Alps, which were several thousand feet higher during the intense cold of the glacial epoch than at present. At all events, it is certain that glaciers once passed over the sites these great lakes now occupy. At Isola Madre, one of the beautiful Borromean islands in the Lago Maggiore, we found the rock surface moutonéed, and stranded on it are fragments of granite and other erratics. The beautiful gardens there, with terraces of orange-trees, lemon-trees, and citrons, pomegranates, oleanders, and magnolias, are situated upon a *roche moutonée* of a great glacier which once swept down

from the Alpine regions of frost. The old ice track is surrounded by the blue waters of the lake; and the nightingale sings in the grove, and the fire-fly flits, on a summer night, above the glacier's ancient bed, while miles away glisten the snows of Monte Rosa. Nor is it only by the Italian lakes we see the remains of glacier action of days long since passed away. Everywhere among the mountains and hills which surround Como and Lugano are the old ice-marks, showing that where now the sheep pastures and wild flowers are blossoming, ice masses crept for long ages over land surfaces which now nourish the fig-tree and the vine. I might give many more examples, but will only allude here to the colossal moraines of a vast glacier which, during the Glacial Epoch, swept down to Ivrea in the plains of the Po, near Turin. Hills higher than the Malverns (1,500 feet), and fifteen miles in length, are there entirely composed of moraine matter, containing enormous erratics brought by an old glacier from the high Alps between Monte Rosa and Mont Blanc. I have observed also that the ice of the Glacial Epoch extended much *farther south* than was formerly supposed. When visiting the shores of the Mediterranean, between Fréjus and Genoa, two years ago, I was struck with the moutonéed appearance of some of the rocks both inland and on the coast. This evidence, too, corresponded with that of the Mentone Caves, where the remains of the mammoth, hairy rhinoceros, marmot, and reindeer showed that the climate there must have once been very different to the present, for all these animals are northern forms, and must have migrated during the Glacial Epoch to the shores of the Mediterranean from their former home in Siberia. Still it was hazardous to speculate upon an accumulation of ice down vales now occupied by olive, orange, and lemon groves, and where even the palm-tree now flourishes. I succeeded, however, with the aid of my friend Mr. Moggridge, notwithstanding the immense amount of atmospheric *débris* with which the country is masked, in tracing glacial action among the picturesque hills which rise above Mentone. To one of these I had the pleasure of conducting my friends Sir W. Guise and Captain Price. This was below the old Castle of Agnesi, where a glaciated, polished, and grooved-rock surface had been laid bare by the quarrying of a mass of angular débris, which had protected the glaciated surface from weathering. I believe this breccia to be of the same age as that which contains the bones of the mammoth, marmot, and bison at the Mentone Caves. Higher up among several cols my friend Mr. Moggridge conducted me to localities where are erratic rocks which he recognised as belonging to the mountains which range in the direction of the Col de Tenda, and could only have been stranded where we found them

by *ice*. Then when we extend our observations to our own country, it wonderfully increases our interest on a tour in North Wales, or Scotland, or among the green hills of Ireland, to mark the signs of vanished glaciers in vales around Snowdon, or Ben Nevis, or by the Lakes of Killarney. I have gathered Alpine flowers from a *roche moutonée*, or clustering beneath a bloc perché, by the waters of Llyn Lydaw (Snowdon), or the summit of Macgillicuddy's Reeks, or away among the deer forests of Sutherland or Ross. Six glaciers once flowed from the mountains of Snowdon down the valleys. The ice has vanished, but the evidence of its former existence still lingers, and few things are pleasanter during a summer's ramble than to trace the old glacier relics to their sources among the wild valleys of wild Wales. But all these proofs of the existence of *Frost action* and *Ice action*, where now there is none, take us back to the *Glacial Epoch*, during a portion of which epoch the great glaciers I have alluded to filled the valleys of Switzerland and Italy, and great erratic masses of rock were carried by land ice, and other erratics were borne by floating icebergs over the seas which then washed over large parts of Europe and North America. So intense was the cold during a portion of the Glacial Epoch that Scotland and North Wales were wrapped in ice as Greenland is now, while the ice covered every hill and valley under one continuous field of ice. Again, we learn that during this epoch there were great changes in the level of land and sea; and that it was during the submergence of large areas in Europe and America that great angular fragments of rocks were transported by icebergs far from the parent rocks to which they belong, and were deposited over wide areas of what now is dry land upraised from those glacial seas.

Such is the history of the ice-borne boulders which overlie the submerged forest of Cromer on the coast of Norfolk. They are wanderers from the rocks of Norway and Sweden, embedded in marine boulder clay; for there are associated with them *marine shells* which lived and died on the sea-bed where the erratics were stranded on the melting of the ice which bore them over the seas. The species of shells, too, testify of the cold climate which affected the seas as well as the land, when icebergs floated down to the latitude of the Norfolk shores. The "Forest-bed" itself is an ancient land surface on which, long ages ago (Pliocene or Preglacial times), there grew large forests which swept over what is now the German Ocean. And these forest lands were inhabited by great quadrupeds—elephants, hippopotami, and rhinoceri—whose remains are found in large quantities in old lake sites of that period, associated with plants and fresh-water shells. These animals belonged to a southern

type of mammalia. The Glacial Epoch came on, and for long ages the forest of Cromer and its lake bed and the skeletons of the animals which had been washed into it, were sunk beneath the waters of the glacial seas and were covered up by these relics of floating ice, the erratics from the distant north. Then, in days long after, we find the forest animals succeeded by a *northern* group of animals—the mammoth, the lemming, the reindeer—which had to migrate for shelter and food from regions which had become uninhabitable through the rigour of intense frost, and where no longer could flourish even the arctic willow or the reindeer moss. And it was then that the vegetation of Great Britain became changed also. Here, then, grew the food of the reindeer, and the arctic birch flourished where many a plant had to yield before the influences of frost; and the dwarf willow, which we now find upon our highest hill summits, among glacier tracks and the groovings of extinct ice, must have grown abundantly by the caves which, in so many parts of England, yield the fossil bones of the reindeer, the mammoth, and the Irish elk.

But if this is the history of *temperate* latitudes, what has been the effect of frost and icework in *northern regions?* There was a time when Greenland and Spitzbergen were not under the dominion of frost as they are now, but when luxuriant forests grew where now sweeps the glacier, and the vine flourished upon sites now sealed by ice which no summers' sun can melt. The researches of geologists have revealed to us that this now icebound continent was, in Tertiary times (Miocene ages), a forest land, stretching away towards the Pole on the north, and to Spitzbergen on the east. Among the trees of this Greenlandic vegetation was a large Sequoia, closely allied to the great Californian pine, the Wellingtonia gigantea, which is found fossil where it grew, its roots in the soil, and around its branches, its leaves, and cones. This tree is very abundant in the lignite beds. The chestnut-tree has been determined, with its flowers and its fruit. There, too, grew the magnolia, of which both the flowers and cones have been preserved, the walnut, the plane-tree, and the vine. There were eight species of oak, the birch, the hazel, and the alder; and beneath these forest trees grew numerous ferns, while the stems of the trees were twined around by the ivy and the vine. Even the fungi on the leaves of certain trees have been detected by their spots and dots and spores, which are determinable under the microscope. Well, then, may we inquire into the *cause* of this wonderful change brought about by the frost and cold of the Glacial Epoch since Miocene times in Polar regions; as well as to the *cause* which brought about the Ice history among our own mountains and valleys of Great Britain.

For a long time geologists have endeavoured to account for this wonderful change of climate by various alterations in the positions of sea and land, elevation of high lands in Polar regions, changes in the flow of the Gulf Stream, and other geographical modifications. Sir Charles Lyell directed attention long ago to the effect which the altered positions of land and sea must have on climate in different parts of the world, and to the alteration which must affect Polar regions, or any other regions, if high mountain ranges were elevated in the place of low-lying plains, or lakes, or seas. In his great work, "The Principles of Geology," he discussed fully the effect of the Gulf Stream in modifying Arctic cold, the effect of the Fohn, or the south wind, now, in melting the ice of Alpine glaciers, and the change brought about by the elevation of the Sahara or Great Desert into land instead of sea, with various other geographical changes and conditions which do, no doubt, affect climate very considerably. But for some time past there has been a strong belief that all these phenomena combined were not sufficient to bring about that intensely cold period which for long ages held our northern hemisphere under the dominion of intense frost, and brought about such extraordinary changes of climate since the Miocene forest days of the Arctic regions. So strongly did Professor Heer feel this that, after several investigations into the history of the fossil trees and plants which formerly grew in Greenland, he said, "We are face to face with a problem whose solution must be attempted, and doubtless completed, by the astronomer." And for some years now, astronomers, mathematicians, and physicists have endeavoured to solve the problem. The possibility of an alteration in the earth's axis was maintained by the late Sir John Lubbock and M. Adhemar, and the changes which might be caused by variations in the eccentricity of the earth's orbit, with other astronomical problems, have been calculated by Mr. Croll, Mr. Stone, and other mathematicians. But I must refer those interested in such questions to Mr. Croll's work on "Climate and Time in their Geological Relations." I will only say that late geological researches do somewhat confirm Mr. Croll's views, which are derived from certain astronomical changes, and the physical conditions those changes produce on the climate of the earth. Mr. Croll believes that there have been *recurring* glacial epochs, that is to say, cold periods followed by warm periods, in both hemispheres, *throughout all geological time.* But geology has for some time past contemplated this question of *recurring* glacial periods. There is undoubtedly evidence of frost and ice action in the transported blocks which occur in conglomerates of the age of the Old Red Sandstone in Scotland. Professor Ramsay was led long ago to infer that ice action was the only

way to account for the transport of large angular erratics which are found in breccias of Permian Age. Mr. Godwin Austen called attention to ice action in France during the Carboniferous Period, owing to similar phenomena presented by carboniferous conglomerates or moraines. The "flysch" of Switzerland indicates the existence of glaciers in the Swiss country during some part of the long Eocene Epoch, while during its earlier ages we know that the nummulitic ocean flowed over sites now elevated into the summits of the Diablerets and the Dent du Midi. So during the Miocene Epoch. In earlier Miocene times we find forests growing in the distant north; while in later Miocene times, or during the deposition of Upper Miocene strata, we know there were Miocene glaciers which bore down great moraines to the neighbourhood of Superga, near Turin. Lastly, the retiring of the colossal glaciers which stranded the Pierre à Bot above Neufchâtel was followed by a warm or interglacial period, with small glaciers; and then *the great glaciers came back again*, and since that have again receded to the present pigmy ice-streams now presented to our observations. These are geological phenomena not to be ignored; and without venturing to pronounce an opinion on the theories of Mr. Croll, I honestly confess many of them will, if they bear the test of criticism, prove a blessing if they enable us to solve some of the more puzzling phenomena presented by geology.

Is it likely or probable, if there was frost and ice action in Scotland and England during Devonian and Permian times, and during the Carboniferous Epoch in France, that at the same period Polar regions were warm, and free from glacial action? Again, Mr. Croll's theory as to the oscillation of the level of the waters of the oceans, caused, as he believes, by the physical effects produced by recurring glacial phenomena, appears to account for some of those changes in the level of land and water which are so difficult to account for when we have to appeal solely to local earthquake movements—earthquake movements in all kinds of places in shifting the land up and down, and down and up again. Again, recurring glacial epochs through all past time would assist us somewhat in accounting for the extermination of whole series of animals, which, as the Frost periods recurred, might have been unable to survive, and during which some species, driven southwards, might have become changed and modified. No one can tell why the mammoth and woolly rhinoceros, which once inhabited Siberia, and later on temperate Europe, in such vast herds, should have died out so as not to have left a single specimen up to historic times, or why the horse and mastodon should have perished in North America. No one can say why no trilobite lived, at all events in these latitudes, through the Permian period, or why

the great Secondary reptilia, and the ammonites and scaphites, which swarmed in the cretaceous seas, should not have lived on to the days of the Tertiaries. It is very easy to say "they died out," but there must be some cause for "the dying out," and it would be satisfactory if we could arrive at some conclusion as to the reason why! Now we can easily conceive that recurring glacial periods must have a considerable effect upon the life of the period, especially when affecting latitudes where for long ages the animals and plants had been adapted to a moderate instead of an Arctic climate. Some species might never become sufficiently adapted to bear the change, however gradual, and, incapable of migration, would perish for ever, leaving only their fossil relics to testify of their former existence. Still, whether these periodical changes of climate, after the lapse of hundreds of thousands of years, have occurred or not, all will perceive what an effect Frost must have had upon that part of the globe which we inhabit, during the Glacial Epoch, and which certainly and assuredly once affected large tracts of country in Europe and America.

A large part of the northern hemisphere covered either by ice like that of Greenland, or by great snowfields and vast glaciers; great herds of animals of various kinds driven towards the south for bare life and sustenance; seas frozen where now the white sail quivers in the wind; rivers bound like iron where now the steamer dashes the waters from her prow; great cities, such as Edinburgh, whose sites were scored by the ice, or grooved by the glacier, or, like the cathedral towns of Gloucester, Bristol, and Worcester, were rolled over by the waters of iceberg-traversed seas—these are no small changes belonging to the history of the land we live in, and were the effects of *Frost*.

And I may take the opportunity of stating that the more I study glacial phenomena in various parts of Europe, the more I am impressed with the belief that the last phase of the Glacial Period, in this country at least, was a period of the return of glaciers in the Highlands, accompanied by great falls of snow on the lower hills, and over Great Britain generally. The melting of this snow, accompanied by a great summer rainfall, producing floods and fresh-water currents, was, I think, the great agent which carried down the gravelly débris which constitutes the angular detritus which is so widely spread over large areas. The same streams, too, which spread out this superficial débris no doubt also washed out of position many of the earlier marine drifts and gravels, and stranded them at a lower level. At the meeting of the British Association held at Birmingham (1865) I endeavoured to uphold these views as applied to the last and overlying drifts of the regions of

Malvern, Siluria, and Wales. Since that I have visited many parts of Europe, and an examination of the phenomena presented, in very many localities, convinces me that the sea has never risen over the land of the interior of Great Britain since the days of the mammoth and the tichorhine rhinoceros. Nay more, I believe that it was a return of glacial climate to these regions which destroyed these animals at last, for on the Malverns there is not a hollow in which the débris I allude to is quarried that does not contain remains of the long-haired elephant and rhinoceros. The old river beds of this last Glacial Period, too, are full of their bones and teeth, rivers flooded by the melting of summer snow and excessive rainfall, and along the banks of which are found marine shells which have been washed into fresh-water beds during what has not been inaptly termed "a pluvial period" by Mr. Tylor.

Every investigation I have made of late years confirms me in the opinion that there has been a return of glaciers among the mountains, and snow and frost in the plains, of Great Britain, with "a wash of many waters" in the summertime, since the days of Cave men and the Cave animals. In long-ago ages the waves of the sea rolled between the shores of the Malverns and the Cotswolds, and deposited with their currents the northern drifts and such marine gravels; but these drifts have been since much disturbed and re-aggregated by fresh-water and subaerial agency, not by the waves and currents of a returning sea. The mammoth and tichorhine rhinoceros, with Palæolithic man, were, I believe, inhabitants of Great Britain when the salt waters of the Severn straits glistened above the sites of Bristol, Gloucester, Tewkesbury, and Worcester, and lived on through all that long period which converted the Severn straits into the Severn river vale. The last of the Mammoths probably saw the configuration of this country much as it is now, but under a return of glacial and pluvial conditions, and under the dominion of rains, and snows, and frost.

HOW HERMIT CRABS GET POSSESSION OF THEIR SHELLS.

By ALEXANDER AGASSIZ.

WHILE tracing the development of one of our species of Hermit Crabs, I raised from very young stages a number of specimens till they reached the size when they need the protection of a shell for their further development. I was of course curious to see how they would act the first time when supplied with the necessary shells. For this purpose, a number of shells, some of them empty, others with the animal living, were placed in the glass dish with the young crabs. Scarcely had the shells reached the bottom before the crabs made a rush for the shells, turned them round and round, carefully examining them, invariably at the mouth, and soon a couple of the crabs decided to venture in, which they did with remarkable alacrity; and after stretching backward and forward, they settled down into their shell with immense satisfaction. The crabs who were so unfortunate as to obtain for their share living shells, remained riding round upon the mouth of their future dwelling; and on the death of the mollusk, which generally occurred soon after in captivity, commenced at once to tear out the animal, and having eaten it, proceeded to take its place within the shell.

It is of course very difficult to apply to Invertebrates many of the laws of natural selection, and thus far we know so little of the habits of most of our marine animals that it is idle to speculate upon the effect of causes which may effectually modify the life of higher animals. In the case above mentioned there is no possible connection between the embryo and the parent to account for the young having learned from the former the use of the shell and its value for his existence. We can therefore only explain the faculty of performing the act as inherited, or else as a simple mechanical act rendered necessary by the conditions of the young hermit crab. This latter seems the more probable case from the nature of the test

of the hermit crab in its younger stages. While the young hermit crab soon after leaving the egg is still provided with its powerful temporary swimming feet, and while the feet of the adult can only be traced as mere rudiments behind them, the whole test of the cephalothorax and abdomen (which are symmetrical) is of considerable consistency up to the last moults preceding the stage when it seeks a shell. At that time the young are no longer symmetrical; the feet, which are now fully developed being largest on the right side, and the abdomen beginning to curve in the same direction away from the longitudinal axis. When the moult has taken place which brings them to the stage at which they need a shell, we find important changes in the two hind pairs of feet, now changed to shorter feet capable of propelling the crab in and out of the shell; we find also that all the abdominal appendages except those of the last joint are lost, but the great distinction between this stage and the one preceding it is the curling of the abdomen: its rings, so distinctly marked in the previous stages, are quite indistinct, and the test covering it is reduced to a mere film, so that the whole abdomen becomes of course very sensitive. It is therefore natural that the young crab should seek some shelter for this exposed portion of his body, and, from what I have observed, any cavity will answer the purpose; one of the young crabs having established himself most comfortably in the anterior part of the cast skin of a small isopod, which seemed to satisfy him as well as a shell, there being several empty shells at his disposal. This mechanical explanation still leaves unanswered the eagerness with which the crabs rushed for the shells, their careful examination of their openings, their taking the animal out and occupying its place; all acts which seem to require considerable intelligence, and to show remarkable forethought.—*Silliman's American Journal.*

REVIEWS.

THE HISTORY OF CREATION.*

ASSUREDLY, if by the term Creation is to be understood the making of the animal and vegetable worlds by the Creator, then the first portion of the title of the present work is slightly out of place. Of course the secondary portion of the title sufficiently explains the nature of the book; but then that in no manner affects the question. The work is essentially one which seeks to bring about the utter annihilation of the Christian faith, and a belief to the fullest extent in the doctrine of Evolution in the development of all animals in a direct line one from the other—the highest having proceeded from the lowest. Of course there is nothing new to be presented in a work like the present one. The writings of Darwin, Huxley, and Herbert Spencer have adequately enough established the doctrine of evolution in the minds of most scientific men. But something further was necessary for the introduction of this matter to the minds of the masses, and for the rendering the subject so general that the reader could grasp it in all its details. A labour of this kind was not of a simple character; it required two separate tasks to be performed by the writer—one to examine the subject in all its aspects, and the other to render his account so clear and intelligible that it would have no difficulty in entering the mind of a popular reader. How has Herr Haeckel done his portion of the task, and how have the editor and translator performed theirs? We think there cannot be a doubt—save on the part of those who are prejudiced on this point—that the author has discharged a most difficult task with conscientious skill and with marvellous ability. On some points we may think he has gone further than absolute testimony would warrant, but these are unimportant; while we cannot help admiring the honest fearlessness with which he expresses opinions which not a few of our English workers hold, but are afraid to acknowledge. In this respect he reminds us somewhat of another

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* "The History of Creation; or, the Development of the Earth and its Inhabitants by the Action of Natural Causes." A Popular Exposition of the Doctrine of Evolution in general, and of that of Darwin, Goethe, and Lamarck in particular. From the German of Ernst H. Haeckel, Professor in the University of Jena. The translation revised by Professor E. R. Lankester, M.A., F.R.S., Fellow of Exeter College, Oxford. In 2 vols. London: King & Co. 1876.

German writer of the same cast of thought—Herr Louis Büchner. Haeckel, indeed, shows very early in his book the tone that he takes throughout, for we find the terse statement on the ninth page that "where faith commences science ends." Of the editor and translator we need only say that their respective duties have been well discharged.

The mode which the author adopts in laying the views of modern natural science before his readers we may understand from a survey of the contents of the several chapters in which these views are expressed. Briefly they are as follows:—Nature and Importance of the Descent-theory; Justification of this Theory according to Linnæus; Cuvier's and Agassiz's View of Creation; Theory of Development, according to Goethe, Oken, Kant, Lamarck, Lyell, and Darwin; Theory of Natural Selection, Inheritance, and Propagation; Laws of Transmission by Inheritance; Laws of Adaptation; Laws of Development of Organic Tribes and of Individuals; Development of the Universe and Earth; Spontaneous Generation, Migration, and Distribution of Organisms; Periods and Records of Creation; Pedigree of the Kingdom of Protista; Pedigree of the Vegetable Kingdom; Pedigree of Animal-plants and Worms; Pedigree of Mollusca, Star-fishes, and Articulated Animals; Pedigree of Vertebrate Animals; Pedigree of Mammals; Origin and Pedigree of Man; Migration and Distribution of Mankind; and, lastly, Objections against and Proofs of the Truth of the Theory of Descent.

These several chapters cover nearly 800 pages of print, so that it must not be said that the author has dealt lightly with his subject. But we think that, in addressing such a work to an English public, Herr Haeckel would have been well advised had he left out a good deal that he has written about the descent of animals. For it must be confessed that anything like a clear line of descent from the Amœba or its congeners to man is absolutely out of the question in the present range of science. We may construct tables of genealogy, as we see the author has often done, but we know that in the course of a few years they are certain to be broken up. Therefore, to this part of the present work we distinctly object; while, of course, we are in absolute agreement with Professor Haeckel as to the whole groundwork of his scheme. And having thus so far expressed our dissent, we may now proceed to notice some parts of this most remarkable book. One of the chapters that strikes us as especially of interest is that in which he shows up the utter fallacy of Agassiz's argument against Darwinism. Agassiz said: "Darwinism shuts out almost the whole mass of acquired knowledge in order to retain and assimilate to itself that only which may serve its doctrine"; to which Haeckel replies: "Surely this is what we may call turning the whole affair topsy-turvey. The biologist who knows the facts must be astounded at Agassiz's courage in uttering such sentences—sentences without a word of truth in them, and which he cannot himself believe! The impregnable strength of the theory of descent lies just in the fact that all the biological facts are explicable only through it, and that without it they remain unintelligible miracles. All our 'laborious knowledge' in comparative anatomy and physiology, and in embryology and palæontology, in the doctrine of the geographical and topographical distribution of organisms, &c., constitutes an irrefutable testimony to the truth of the theory of descent." This is strong language, but it certainly

is not stronger than was meet for Agassiz's reckless assertion of falsehood.

Another passage in this work which strikes us as being especially interesting, is that from a letter of Darwin's to Professor Haeckel, in which the former traces out the mode in which he came to hit on his great discovery, that of Natural Selection. He says: "For some years I could not conceive how each form became so excellently adapted to its habits of life. I then began systematically to study domestic productions, and after a time saw clearly that man's selective power was the most important agent. I was prepared, from having studied the habits of animals, to appreciate the struggle for existence, and my work in geology gave me some idea of the lapse of past time. Therefore when I happened to read 'Malthus on Population' the idea of natural selection flashed on me." This fact is important to English readers, as we think it has not been mentioned in any of Mr. Darwin's own books.

On looking for further points on which to dwell in our notice of this excellent work we have come across one which is somewhat painful to read, as the supposed facts on which it rests are now admittedly mistaken ones. It is as to the Bathybius. Professor Haeckel, writing before Mr. Huxley's renunciation of this organism, describes it as being an unquestionable animal. Our readers are of course aware that Professor Huxley has some six months since completely given up the animality of this quasi-organism; it is, therefore, the more to be wondered at that Mr. Lankester should have allowed Haeckel's remarks to stand unqualified in the first volume, on p. 184, and in the second volume on p. 53, and have merely added a footnote on p. 344, vol. i., which says: "We must wait for fuller information on the subject of Bathybius at the hands of the naturalists of the *Challenger* expedition, before accepting it finally as a distinct organism." We should have expected that he would have removed the passages in which Professor Haeckel has so clearly given his assent to a doctrine whose chief advocate has torn up his brief. One explanation of this apparent neglect may be that the work had gone through the press at the time that Professor Huxley had given in his recantation, and if this be so, Mr. Lankester's note was but a foresight of the ultimate result.

After giving many examples of rudimentary organs being preserved in animals which have no purpose they can fulfil, and of the absence of organs being accounted for by natural selection—as in the case of the beetles of Maderia, which are almost all wingless—the author gives an excellent plate showing the development of the tortoise, chick, dog, and man. This shows very well to the general reader the close resemblance of these four distinct organisms at an early period of life; first, of the tortoise, dog, and man, of four weeks, and the chick of four days; and second, of the tortoise and dog, six weeks, the chick of eight days, and man of the eighth week. The subject of the wonderful relation between invertebrata and vertebrata, which was first indicated in the year 1867 by Kowalewsky's researches, has been especially dwelt on by the author of the present work, who has given us two capital plates illustrating the relation between the Ascidian Phallusia and the common Lancelet. In the first of these, the immature animals are contrasted, and the comparison shows the extraordinary relation which exists

between the molluscan and vertebrate animals. Plate xiii. represents the two perfect types side by side; and though of course there are distinctive marks, yet the resemblance of the two organisms is singularly forcible. We are glad to observe too that the author draws the following conclusion to his remarks on this subject: "Of course we do not mean to say by this that vertebrate animals are derived from tunicate animals, but merely that both groups have arisen out of a common root, and that the tunicates of all the invertebrata are the nearest blood-relations of the vertebrates."

In reference to the question, from which of the quadrumana did man originate? Professor Haeckel thinks that the "*human race is a small branch of the group of Catarrhini, and has developed out of long since extinct apes of this group in the old world.*" And when on this subject he refers to Professor Huxley's remarks,* which show that man, is nearly as much as the ape, a foot-handed animal; for that various tribes of men, the Chinese boatmen, the Bengalee workman, and the negro, when climbing, use the great toe in the same manner as the monkey, and therefore that the possession of *only* a single pair of hands is not to be looked on as a character of the human race. He also points out a fact necessary to be observed by unscientific people, viz. that none of the man-like apes are to be regarded as the parents of the human race, but that the "ape-like progenitors of the human race are long since extinct." In concluding his work Professor Haeckel remarks on the desire of some who are not actually opponents of the doctrine of Descent. "They await," he says, "the sudden discovery of a human race with tails, or of a talking species of ape." But such manifestations, as the author very properly observes, would not furnish the proof desired, and unthinking persons would be provided with as satisfactory (?) arguments as they nowadays employ in hurling their defiance against all who are evolutionists.

And now we must bring our notice of this most admirable work to a close; and while we bid adieu, or rather *au revoir*, to the author, we must say a word in thanks to the publishers, who have not only done their part of the work "most excellently" well, but who have shown a courage that many would have shrunk from in bringing out this book in its English guise.

THE RECORD OF GEOLOGICAL SCIENCE.†

THIS work forms a valuable addition to geological literature, and supplies a want that must have been frequently felt by the student and worker in geological science. Although it includes only notices of works relating to geology during one year (1874), it comprises more than 2,000 separate entries of books, memoirs, maps, and sections which have appeared in that

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* "On our Knowledge of the Causes of the Phenomena of Organic Nature." By Professor Huxley, F.R.S. London: Hardwicke. 1862.

† "The Geological Record for 1874. An Account of Works on Geology, Mineralogy, and Palæontology published during the year." Edited by W. Whitaker, B.A., F.G.S. London, 1875.

period, not only in this country but throughout the world where geological research is cultivated. When it is considered the vast range of literature that had to be consulted, not always easily accessible, and in different languages, geologists cannot but feel grateful to Mr. Whitaker and his well-supported co-editors for the labour and energy they have bestowed in thus making as useful and complete as possible the first volume of the "Geological Record." The work, with the addenda, extends to nearly 400 pages, and is accompanied by a copious index. The subjects are classified under different heads—Descriptive, Statigraphical, Physical, Economical Geology, Petrology, Mineralogy, and Palæontology; and the latter (which might have been further divided) under Vertebrata, Invertebrata, and Plants. Under each heading the authors' names are arranged alphabetically, followed by the title of their works, and generally by a concise description of the contents. The maps are in the alphabetical order of places, so that the whole forms an easy and useful volume of reference.

ON RE-FORESTING IN FRANCE.*

THE severe inundations and the torrential floods which have frequently occurred in the South of France and other districts of Europe, causing extensive destruction of life and property, have drawn considerable attention to the subject, not only recently but for many years past, and have been the object of legislative measures by the Government of the former country. It is an inquiry of much importance in a scientific and economical point of view, both as regards the causes of their origin and the means to be adopted for preventing or modifying the same.

That the entire destruction of forests is one of the primary causes of torrents there can be little doubt; but besides the loss of the woods there is also the action of the torrents, in denuding the vegetable soil, in covering up the lower grounds with deposits which alter their nature, in filling up and diverting watercourses, and other injurious effects. To obviate these effects many suggestions have been advanced by various writers, especially the work of Surell, in 1841, "Étude sur les torrents des Hautes-Alpes," and to which may be traced the commencement of the works of *reboisement* and *gazonnement* which are now being carried on in the Alps, the Cevennes, and the Pyrenees, by the re-clothing the mountain-sides and brows with trees, herbage, and bush. That the remedial measures may be expensive, and the present outlay large, so also must be losses caused by these destructive floods, but the ultimate gains must be great, for it cannot be doubted that *reboisement* will be an invaluable advantage to those districts subject to torrential floods. Dr. Brown has supplied in this work much practical and useful information compiled from the numerous authorities who have treated of

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* "Reboisement in France; or Records of the Re-planting of the Alps, Cevennes, and the Pyrenees with Trees, Herbage, and Bush." Compiled by J. C. Brown, LL.D. London, 1876.

this subject, and especially what has been done in France in carrying out works of *reboisement*, with a view to preventing and arresting the destructive consequences and effects of torrents, and the results which have followed.

THE CRETACEOUS VERTEBRATA OF NORTH AMERICA.*

THIS volume is another valuable contribution to the Geological Survey of the Territories, under the direction of Dr. Hayden; and, considering how much important and interesting matter, both geological and palæontological, has been brought out by the arduous and energetic labours of the Director and his able collaborators during the progress of the survey, we cannot but hope and wish that it may be as successfully continued, by receiving the liberal support of the Government, as heretofore; not only that the geological nature of the district should be well known in its immediate economical bearings, as advancing the interest of the State, but that by the full publication of the details obtained, European geologists may be enabled to compare the lithological structure and palæontological character of the American strata with those in other areas considered to be either contemporaneous or homotaxious with them, and thus arrive at a more enlarged knowledge of the different physical conditions and the distribution of life which obtained in different parts of the earth's surface during the time assumed to belong to the same great geological period. In the latter direction the memoir on the Cretaceous Vertebrata, by Professor E. D. Cope, is very important, for palæontology lies at the very foundation of geological science; and, therefore, as a contribution towards the solution of the numerous problems involved in the geological structure of the Western Territories, as well as the unfolding of the ancient life, this work must be considered of the highest rank, and also forms a companion volume to that on the Cretaceous Flora by Professor Lesquereux, already noticed ("Pop. Scien. Rev." vol. xiv. p. 411).

Independently of its palæontological value it may be commended as a work of art, for it is illustrated by nearly sixty well-executed plates, with explanations and descriptions, together with a synopsis of the known cretaceous vetebrata of North America, which now amount to 253 species of birds, reptiles, and fishes. In this list is included numerous remains from the Fort Union beds, or lignite group, considered by Professor Cope to be of cretaceous age, but which are referred by other authors to the tertiary epoch or transition series of Hayden, from their containing a tertiary flora associated with a cretaceous fauna, thus inferring—in this area at least—there is no real physical break in the deposition of the sediments between the well-marked cretaceous and tertiary groups. With regard to the fishes of the Niobrara group, it is important and interesting to observe that nearly all

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* "Report of the United States Geological Survey of the Territories." F. V. Hayden. Vol. ii. "The Vertebrata of the Cretaceous Formations of the West." By E. D. Cope. Washington, 1875.

the genera have been also obtained from the chalk of Europe, thus pointing to the synchronism, as generally understood, between the chalk-formations of Kansas and England. Another largely developed American group are the reptiles referred by Professor Cope to his order *Pythonomorpha*, including the mososauroid forms and their allies, which according to him represent an order of reptiles distinct from any other, and present characters which ally them to both serpents and lizards. This order of reptiles attained a predominant importance during the Niobrara epoch of the cretaceous period, as is indicated by the great profusion of individual remains and specific forms. Although occurring in America, wherever the cretaceous formation appears, they are more numerously represented in Kansas than elsewhere. The seas of the American continent appeared to be the home of this order, while they were comparatively rare in those of Europe, for in the latter country only four species have been recognised. Geologists will be grateful to Professor Cope for this final but elaborate report on the vertebrate fauna of America, showing that the cretaceous ocean of the West, teeming with an abundant and vigorous life, was no less remarkable for its fishes than for its reptiles.

ANIMAL PARASITES.*

THE elder Van Beneden has passed so completely out of our recollection that we had imagined the present work was written by his son, a distinguished follower in his father's footprints. It is so many years since the late Dr. Lankester introduced to our notice in one of the Ray Society's volumes the researches of Van Beneden and Küchenmeister, that we had imagined that the Belgian professor had "gone the way of all flesh." We are delighted that our ideas were mistaken ones, and that the first experimenter on the subject of the Entozoa, and the demonstrator of the fact that the *cysticercus* of pig becomes converted into the *tænia solium* or common tape-worm of man, has now written a book on the subject of animal parasites. There are very few outside the scientific world who have any idea of the vastness of the subject of animal entozoa. There is, in point of fact, no animal which has not got its parasite. Some, indeed, have many. So that it is not at all an erroneous statement, that animal parasites are the most numerous group of beings in existence. It is clear therefore that M. Van Beneden has had a very wide field before him from which to prepare the book now under notice. Indeed, we should have thought that it would have been too vast a subject for anyone to attempt the treatment of in a single volume like that before us. However, there are two modes of dealing with the subject, the scientific and the popular; in the one of course the writer must treat at length, fully and completely, with each animal under discussion; in the other he can pass in a sketchy manner along the field of his discourse, and can touch lightly on the more complex subjects, or enlarge

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* "Animal Parasites and Messmates." By P. J. Van Beneden, Professor of the University of Louvain, Correspondent of the Institute of France. With 83 Illustrations. London: King & Co. 1876.

on those best suited to the public mind. It is the latter view which M. Van Beneden has taken. And in our opinion the choice has been a perfectly wise one. For it must be remembered that the book is not a strictly scientific one, but, on the contrary, is intended for the man who has scientific tastes without scientific knowledge. Therefore the whole style and tone of the present work are in adaptation to the wants of the general reader. If there is one defect in M. Van Beneden's observations it is that he seeks in some cases to be pleasant in style at the expense of conciseness of language and clearness of thought.

In the first instance we must take exception to the author's division of parasites, which seems to us to be a perfectly artificial one, and one too which, however well it may look on paper, does not hold water as a practical mode of grouping. He divides all parasitic animals into *Messmates*, *Mutualists*, and *Parasites*, and the last he groups as parasites free during their whole life, parasites free while young, parasites free when old, parasites that migrate and undergo metamorphoses, and lastly parasites during their whole life. Under these several headings he supplies a vast amount of information, occasionally illustrated—but it must be confessed imperfectly—in which he gives an excellent and popular account of the singular vagaries of the various parasites. Talking of the wanderings of the individuals which make up an entire cestoid worm, he says that recently Herr Leuckart, in concert with M. Mecznikow, has discovered "transmigrations of worms accompanied by changes of sex; that is to say, they have seen nematodes, the parasites of the lungs of the frog, always female or hermaphrodite, produce individuals of the two sexes which do not resemble their mother, and whose habitual abode is not within the lungs of the frog, but in damp earth. In other words, let us imagine a mother born a widow, who cannot exist without the assistance of others, producing boys and girls able to provide for themselves. The mother is parasitical and viviparous, her daughters are during the whole of their life free and oviparous."

M. Van Beneden might well have given us more information on the subject of the *Gregarinæ* than he has furnished; the paragraphs supplied being extremely brief and not by any means exhaustive. However, with this and one or two other instances of neglect, the author has performed a difficult task with admirable skill. He has told us many wonderful and truthful tales, and has brought so much humour to bear on the subject, that he has made many unusually dull questions sparkle with the wit and vivacity with which he has surrounded them.

FIRST BOOK OF ZOOLOGY.*

THERE is in this little work a considerable amount of originality exhibited, both in composition and illustrations. The book too is luxuriously got up as to type and paper. With these remarks our friendly criticism

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* "First Book of Zoology." By E. S. Morse, Ph.D. Henry S. King & Co. 1876.

ceases. There is an absence of plan in the construction of the work. Details oft of no importance are too much attended to. Withal the book is styled a work on zoology, yet the vertebrata occupy only about ten or twelve pages. Then there is an utter absence of anything like classification. Finally, the author's list of works to be referred to shows an utter misconception of the nature of the wants of the student. We cannot recommend the work to any but the dilettante teacher of zoology.

POPULAR NATURAL HISTORY.*

OF the two little books that we have before us we take that which is written by the woman to be the best, as it certainly is the most original. Indeed, the "Dwellers in our Gardens" is in many respects a clever book, in which many points in natural history are exceedingly well told in pithy language, and withal in a style which is quite commensurate with the intelligence of the class to which it is addressed. It is specially interesting from the fact that it possesses as a frontispiece a plate which is apparently borrowed from the "Curiosities of Entomology," and which shows us at a glance several remarkable instances of insect disguises. And by insect disguises we mean some of those resemblances of animals to plants which serve to preserve the former, by the natural preservation which they afford to them from their enemies. Many of these curious facts in natural history have been indicated by Darwin, Wallace, Belt, Bates, and others, and in the works of few of these distinguished writers are the curious facts of animal disguises better illustrated than in Sara Wood's little volume. Her several chapters are all taken up with the ordinary animal occupants of our country gardens. But there are little bits here and there that show us the writer's observations as a naturalist. This is especially to be seen in the remarks on the spinnerets of the spider, in which the structure of the web and apparatus for its manufacture are very fully and intelligently explained. In the chapter which is devoted to the lepidoptera we find a full account of the microscopic scales of the butterfly, and of the numerously curious forms of the eggs in the lepidoptera. The other chapters on bees, aphides, birds, and frogs are likewise interesting; and, together with the charming coloured plate of birds, birds' eggs, moths, and butterflies, form a volume of pleasant and useful material for the young. Indeed, we have never seen a better coloured plate than that of the golden-crested wren, which faces p. 107 of this excellent little work.

Mr. Houghton's book, which is issued by the same house as the above, and which is got up in a somewhat similar style as regards paper, print, and illustration, is not so original, but is by no means a bad essay on the subject.

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* "The Dwellers in our Gardens: their Lives and Works." By Sara Wood. London: Groombridge. 1875.

"Sketches of British Insects: a Handbook for Beginners in the Study of Entomology." By the Rev. W. Houghton, M.A., F.L.S. London: Groombridge. 1875.

Of course there is nothing new in its pages, but the facts of entomology are pleasantly put, and the introduction on the anatomy of the animals is not by any means a bad sketch of insect structure. As a book on entomology, we think it a very excellent work to place in the hands of a beginner.

FOOD AND ITS ADULTERATIONS.*

WITHIN the past ten years the subject of adulteration of food has acquired an immense importance, from the fact that it has been during that time possible to detect any infringement of the law, and equally possible to punish—and that most severely—those who have been guilty of the act of adulteration. During that period various books on the subject of the detection of impurities in food have been published, among the first of which we should certainly place the work on hygiene of the late Dr. Parkes, of Netley. Indeed, we know of no other writer who treated generally on the subject, with the exception of the author of the present volume, who published, eighteen years ago, a work entitled "Adulterations Detected in Food and Medicine." Now, however, Dr. Hassall has again come to our assistance, and has brought out a new edition of this essay; and he has so modified his original remarks, and has added so largely to the contents, that we think he is perfectly justified in issuing the work under a new title, that of "Food: its Adulterations and the Methods of their Detection." To do anything like a fair review of this work would be impossible in anything less than a couple of sheets of matter. And that this is so will be at once evident when we state that it consists of nearly 900 8vo. pages of closely-printed matter. We shall therefore only touch on some two or three points, after giving a sketch of the book's general plan. Dr. Hassall's work is divided among fifty different chapters, as follows:—Firstly, we are treated to some observations on the subject of food in regard to its functions and quantity, and then we have a chapter devoted to the different modes of preserving food. After this come the following chapters, in each of which the author deals fully with his subject, showing us the characters of both the normal and adulterated article, and offers illustrations most of which are reliable, though in one or two instances they are ideal—that is to say, that *one* illustration represents every conceivable form of abnormal variation: water, tea, coffee, chicory, cocoa, sugar, honey, flour, bread, oatmeal, arrowroot, sago, tapioca, proprietary alimentary preparations, milk, butter, cheese, lard, isinglass, gelatine, unwholesome and diseased meat, potted meats and fish, anchovies, bottled fruits and vegetables, tinned vegetables, jellies and preserves, mustard, pepper, cayenne, spices, curry-powders, turmeric, liquorice, annatto, vinegar, pickles, lemon and lime-juice, sauces, aërated waters, malt beverages, cider and perry, wine, and, lastly, spirituous liquors. Then,

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* "Food: Its Adulterations and the Methods for their Detection." By Arthur Hill Hassall, M.D., M.R.C.P., &c. &c. Illustrated by upwards of 200 wood engravings. London: Longmans. 1876.

after the discussion of these several chapters, comes one on the utensils used in the preparation and storage of food, and on various methods of detection of adulterants. These are followed by a general summary of adulteration; and last, but by no means least, comes a chapter useful to both the medical officer of health and the chemical analyst, and that is the Act for the sale of food and drugs, which was issued in 1875.

Out of this enormous list of subjects it is hard to choose any, for all appear of almost equal import. However, we may select a few, and first of water. This is a form of food which all, whether high or low, have to partake of, and it is therefore of the utmost importance that we should have the means of detecting the presence of impurity. Indeed, it is the more so from the fact that it is now believed to be the medium by which we take into our bodies the contagions of cholera, typhoid fever, and possibly also scarlatina. We are glad to see, therefore, that Dr. Hassall has given ample space to the treatment of this subject. His remarks cover no less than 77 pages, and deal with every possible question connected with the purity and impurity of almost every conceivable form of water. From these we select his remarks on the question of filtration. For it is by the filter alone that people generally have any control over their drinking-water, and it is of great importance that attention should be paid to the following statements:—

"Of course the powers of all filters are limited, and they speedily become spoiled when too much work is thrown upon them at one particular time—that is to say, when water containing a large quantity of organic matter, say, six or eight grains per gallon, is rapidly passed through them. In this case the requisite time is not afforded for the due action of the filters, which become simply clogged; but when water containing only a moderate amount of impurity, as one grain per gallon, is passed through, then the action of the better filters, especially those containing charcoal, is not only satisfactory but continuous." In regard to the mode of cleaning filters, which should be done often, contrary to the usual practice of keeping them in perpetual action from year's end to year's end, Dr. Hassall quotes as follows from Dr. Parkes' able treatise: "Every two or three months (according to the kind of water) four to six ounces of the pharmacopœial solution of potassium permanganate, or twenty to thirty grains of the solid permanganate in a quart of distilled water, and ten drops of strong sulphuric acid, should be poured through, and subsequently a quarter to half an ounce of pure hydrochloric acid in two to four gallons of water. This will aid the action of the permanganate, and assists in dissolving manganic oxide and calcium carbonate. Three gallons of distilled or good rain-water should be then poured through, and the filter is fit again for use." We think it a pity that Dr. Hassall reproduced his old cuts representing the fresh-water animals, as they convey incorrect ideas at the present time. And we would also allude to the objectionable analysis of Messrs. Allsopp's beer, for which Dr. Hassall has had to offer a public apology.

Under the head of milk and its adulterations we find some very useful practical hints, as well as various methods of analysis. But we are not in agreement with the author in thinking that sheep's brains are used in the adulteration of milk. However, he gives an illustration showing the ap-

pearance of milk which does possess this form of adulterant. We are surprised to find how frequently anchovies are adulterated; in fact, how very seldom we can obtain them pure and undefiled. From an examination made by Dr. Hassall it seems that of twenty-eight samples of anchovies, "seven of the samples consisted entirely of Dutch fish, two consisting of a mixture of Dutch fish and anchovies, and that the brine in twenty-three of the samples was charged with either *bole Armenian* or *Venetian red*." The last subject to which we shall refer in this notice is Dr. Hassall's observations on the subject of beer. Of this drink he records the following different kinds of adulterants: water, cane-sugar, liquorice, burnt sugar, gentian, wormwood, quassia, calumba root—detectable by Mr. Sorby's spectroscope—chiretta, bitter orange-peel, camomile, picric acid, picrotoxin, nux vomica or strychnine, opium, tobacco, ginger, capsicum, sulphate of iron, and alum.

The classified list which the author places at the end of the work will be found highly useful by the analyst. In conclusion, we may say that we regard this work as in every way a most thoroughly satisfactory one, and without which the analyst's library would be most seriously deficient. We have only to regret that the author has adopted the system of inducing persons to advertise their goods at the conclusion of his volume. The practice is somewhat novel, and smells strongly of the shop.

NATURAL SCIENCE: WHAT IT IS.*

WHEN we first took up this book we exclaimed, with a feeling somewhat of contempt, "Here is another Mrs. Somerville!" But before we had read the first half-dozen pages we laid it down with an expression of admiration of the very wonderful powers of the writer. And our opinion has increased in intensity as we have gone on through the work, till we at length have come to the conclusion that it is a book worthy of being ranked with Whewell's "History of the Inductive Sciences;" and it is one which should be first placed in the hands of everyone who proposes to become a student of Natural Sciences, and it would be well if it were adopted as a standard volume in all our schools over which the School Boards have authority. The writer has traced in a most clear style the progress of Natural Science—including, of course, a certain amount of Physical Science—from the times of Pythagoras and the other Greek philosophers, before the Christian epoch, down through the period of the Ptolemys; then through the time when science was unknown, save to the Arabs, who first introduced algebra to the world, to the period of the so-called Middle Ages, when it began to be pursued by some few Europeans. Beginning then at the sixteenth century, she has given us an admirable account of the progress made in that age, and in the seventeenth and

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* "A Short History of Natural Science, and of the Progress of Discovery from the Time of the Greeks to the Present Day; for the Use of Schools and Young Persons." By A. B. Buckley. With Illustrations. London: John Murray. 1876.

eighteenth of the Christian era. Next comes the nineteenth century, the especially scientific age, and this occupies far more than one-third of the entire volume. Of the plan of the book we have only to speak in the most favourable terms. And of the author's style we cannot speak in terms which are too warmly praiseworthy. Let us take a couple of examples; one from the early portion of the book, and another having to do more with our own time. And first let us see how she explains the well-known experiment of Archimedes when he was discovering specific gravity. It is not an easy point to explain lucidly to one who is totally ignorant of science, yet we think Miss Buckley has been wonderfully successful. Speaking of Archimedes' well-known excitement and his cry of "*Eureka, Eureka!*"* she asks, "What had he found? He had discovered that any solid body put into a vessel of water displaces its own bulk of water, and therefore, if the sides of the vessel are high enough to prevent it running over, the water will rise to a certain height. He now got one ball of gold and another of silver, each weighing exactly the same as the crown. Of course the balls were not the same size, because silver is lighter than gold, and so it takes more of it to make the same weight. He first put the gold into a basin of water, and marked on the side of the vessel the height to which the water rose. Next, taking out the gold, he put in the silver ball, which, though it weighed the same, yet, being larger, made the water rise higher; and this height he also marked. Lastly, he took out the silver ball and put in the crown. Now, if the crown had been pure gold the water would have risen only up to the mark of the gold ball; but it rose higher, and stood between the gold and silver mark, showing that silver had been mixed with it, making it more bulky. This was the first attempt to measure the *specific gravity* of different substances."

We have chosen the above passage as an illustration of the author's admirably clear and simple style, and of her success in rendering what is not the simplest matter in the world perfectly intelligible to an average reader. And it is the same fashion in which she deals with many other and more complex problems. But what we most admire about the book is the utter absence of the "goody-goody" tone, which most women and not a few men feel it incumbent on them to introduce when attempting to explain any fact in science to those whom they consider to be beneath them in intellectual knowledge.

We shall only select another passage from Miss Buckley's book, though had we space we should quote it more abundantly. It is that in which she explains to her readers—supposed to be children—the effects of natural selection. Now, this is not an easy matter even for an adult to explain, for we doubt not there are many even among our better educated friends who would feel themselves utterly "at sea" if such a question were proposed to them. Hear how the authoress explains the matter. "Mr. Huxley tells us that a single plant producing fifty seeds a year would, if unchecked,

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* It was, of course, in reference to the question put to him by the king, if he could find out whether the jewellers had, in making the crown, kept back some of the gold, and supplied its weight with some other metal.

cover the whole globe in nine years, and leave no room for other plants. It is clear, therefore, that out of these numbers millions must die young, and it is only the most fitted in every way that can live and multiply." Mr. Darwin "tells us that the heartsease and the Dutch clover, two common plants, only form their seeds when the pollen is carried from flower to flower by insects. Humble-bees are the only insects which visit these flowers; therefore if the humble-bees were destroyed in England there would be no heartsease or Dutch clover. Now, the common field-mouse destroys the nests of the humble-bee; so that if there are many field-mice the bees will be rare, and therefore the heartsease and clover will not flourish. But again, near the villages there are very few field-mice, and this is because the cats come out into the fields and eat them; so that where there are many cats there are few mice and many bees, and plenty of heartsease and Dutch clover. Where there are few cats, on the contrary, the mice flourish, the bees are destroyed, and the plants cease to bear seeds and to multiply." We can quote no further, but we may state that the writer goes on to show that the scheme may be extended still more by imagining that certain mice are, through their peculiar odour, disliked by cats, and thus a new race arises which destroys the bees, and thus destroys the clover and heartsease. and then she goes on to consider the possibility of a new race of plants coming into existence through the influence of moths, which would by fertilising those plants with peculiar drooping petals enable those only to be propagated.

The matter we have taken from Miss Buckley's book is adequate to prove that not only does she possess the ability to teach, but that she also has the knowledge from which to draw her various lessons. And in concluding this very imperfect notice of her labours we must express the hope that, notwithstanding Sir J. Lubbock's recent failure in the House of Commons to modify the scheme of the School Board, some effort may be made to introduce her volume very largely to the educational classes.

THE GREAT DIVIDE.*

THE Earl of Dunraven has given us a clever sketch of his excursion to the Yellowstone territory of North America—to that portion of the most majestic scenery of the United States which has recently been converted into public property by a bill passed in the Senate about four years since. The term *Great Divide* he has chosen because he considers that the line of mountains of which this Yellowstone *Park* (as it is, we think, somewhat absurdly styled) forms part, is a natural division of the continent into two portions, an eastern and a western. He has thus termed his book "The Great Divide," an expression, we opine, that very few of his readers will guess the significance of. The Earl has done wisely, in presenting his adventures to

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* "The Great Divide: Travels in the Upper Yellowstone in the Summer of 1874." By the Earl of Dunraven. With Illustrations by Valentine W. Bromley. London: Chatto & Windus. 1876.

the public, to avoid anything like exaggeration. Indeed, he himself states that he has met with nothing extraordinary in the course of his explorations. No tomahawking or hairbreadth escapes from Indians are to be found in these pages. Indeed, on the contrary, with two or three exceptions, the pages of the volume before us do not record anything that might not have occurred to a person travelling in the Austro-Italian Tyrol. Though, of course, had the Earl of Dunraven taken a different route—which at the time was blocked, owing to disputes between the American Government and the Indians—he might not have had an opportunity of discoursing so pleasantly on the subject of the Great Divide. Still his account is full of interesting matter, and in no respect is it so good as in description of scenery. The author appears to have a wonderful power of sketching out in words the scenes which passed before him. And in no case is this better shown than in his graphic description of the wondrous view obtained from Mount Washbourne, the peak *par excellence* of the Yellowstone mountains. "From it has been traced out the geography of the country. The main divisions, the great centres of trade, together with the natural features that sway the fates of men and nations, radiate thence; and by a citizen of the United States the spot should be regarded as sacred ground. From it he can overlook the sources of the Yellowstone, the Wind River, and the Missouri, and of the Snake and Green Rivers, principal tributaries the one of the Columbia, the other of the Colorado. These waters flow through every variety of climate, past the dwellings of savage hordes and civilised nations, through thousands of miles of unbroken solitude, and through the most populous haunts of mercantile mankind; now shaded by the great pine-trees of the forest, again shadowed by tall factory chimneys; here clean and undefiled from the hand of Nature, there turbid and contaminated by contact with man; and from Mount Washbourne I believe that the head-waters can be seen of mightier rivers—rivers passing through more populous cities, through the hunting-grounds of more wild tribes, through greater deserts, through countries more rankly fertile, through places more uncivilised and savage, by scenes stranger and more varied, than can be viewed from any other point on the surface of this earth."

The engravings are numerous, but in most instances they lack the interest that would have arisen had they represented the country rather than the explorers themselves. Still they are not without cleverness both of design and execution. In some cases, too, the author's style is coarse, and we had almost said ungentlemanly. For instance, in describing the habits of a female Indian he observes: "For his helpmate is reserved a smaller but more vivacious species of game, in the pursuit and capture of which she must take great delight, to judge by the interest portrayed in this case on the countenance of the lady, as with unerring eye and unfaltering hand she through the thick tangles of her husband's hair hotly pressed the bounding fugitive, or like the relentless blood-hound surely tracked to his lair the slow-crawling and unmentionable one." However, the defects in the volume are small; and though the book can hardly be termed scientific, still it does contain some references to geology and chemistry, and it is an honest and clever account of the author's experiences of men and things.

CAROLINE HERSCHEL.*

THERE are few who have any interest in astronomy who will not be delighted to read this charming life, told as it is nearly in the words of ts subject. It is, in fact, almost an autobiography, for the few remarks that Mrs. John Herschel has added do little more than serve to bind the various letters and statements of Caroline Herschel into something like a continuous record. What a wonderful old and young woman she appears to have been, and how marvellously she was devoted to her brother William, who was first musician and then astronomer! She, says Mrs. Herschel, "had been his helper and assistant in the days when he was a leading musician; she became his helper and assistant when he gave himself up to astronomy." We think that Mrs. John Herschel has done well to have handed us down the life of her distinguished ancestor, as well because her subject deserves a biography as because that biography is full of interesting facts to the lovers of the stars. We had almost forgotten to mention that the frontispiece is an excellent portrait of the old lady at the advanced age of ninety-two.

ARCTIC YACHTING.†

WITH the assistance of Dr. W. S. Livesay, who has done the editing and illustration of this work, Mr. Lamont has given us an instructive sketch of his five voyages of sport and discovery in the districts of Spitzbergen and Nova Zembla. There is not much to be said about the natural history of the book, save that some capital illustrations are given of the walrus, the bear, and the reindeer; but as a work of travel the work has many points of interest, though not of novelty. Still it is a volume which from its illustrations alone, which are both numerous and good, must have an interest for all, and from its matter, which is told in a graphic manner, must especially excite those who care about adventure. In some cases the author was in extreme danger, and many incidents he records remind us of our brave sailors who are at the present moment exploring the polar regions. Readers who are members of the Royal Geographical Society should purchase this volume.

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* "Memoir and Correspondence of Caroline Herschel." By Mrs. J. Herschel. With Portraits. London: J. Murray. 1876.

† "Yachting in the Arctic Seas; or, Notes of Five Voyages of Sport and Discovery in the Neighbourhood of Spitzbergen and Novaya Zemlya. By J. Lamont, F.G.S. Edited and Illustrated by W. Livesay, M.D. London: Chatto and Windus. 1876.

THE THREE HEAVENS.*

THOSE who like their science worked up with a certain amount of Scripture references will take an especial delight in the work which is entitled the "Three Heavens." It is the reprint of a series of essays which have appeared in a periodical termed the "Sunday Magazine." The writer, who is a son of the eminent Irish surgeon, Sir P. Crampton, has a very perfect knowledge of his subject, and he writes well and illustrates his remarks very happily. We do not know that we can say anything more in praise of his book; whilst of his many theories in regard to miraculous clouds, *moral* thunderstorms, Scriptural evidence of heaven, and unnatural separation of science and religion, we suppose we had best leave the reader to form his own opinion.

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* "The Three Heavens." By the Rev. Josiah Crampton, M.A., Rector of Killesher. London: W. Hunt & Co. 1876.

SCIENTIFIC SUMMARY.

ASTRONOMY.

A WINTER of such peculiar and continuous obscurity could not be expected to testify to much astronomical progress as far as observation is concerned. From Mr. Gledhill's analysis of the weather, recently communicated to the "Astronomical Register," it appears that at Mr. Crossley's observatory, near Halifax, there were 2 very fine, 9 good, 22 fair, and 21 bad nights during the year 1875; and matters have certainly not been mending since in England. We can only hope that observers in other parts of the world may have been more favoured.

The Sun has continued in a very undisturbed condition, though one large spot, visible to the naked eye, was reported in February, which may be the precursor of renewed activity. From an ingenious method of registering isothermals on the sun's disc by the blackening of the double iodide of copper and mercury, Professor Mayer has been led to the conclusion that areas of uniform temperature exist on the sun's surface, the position of which is subject to continual variation. Professor Langley thinks that the light of the dark nuclei of solar spots equals at least 5,000 times that of the full moon.

Venus.—This beautiful planet has been gradually increasing in brilliancy and altitude, and is now in a very good position for observation. On several occasions during the present season Mr. Dennett has distinctly traced spots on her disc with a $2\frac{1}{4}$-inch achromatic; and on Feb. 29 the present writer was able to confirm their existence with a powerful silvered-glass reflector; though they were so faint that they would have escaped any cursory examination, and could not have been delineated. It is very desirable that more attention should be paid to this body, so conspicuous, so near, and yet so little known.

The Moon.—A new and important work on our satellite may be expected from Mr. Neison, who has for some time paid much attention to selenography. He has taken very accurate micrometrical measures of the position of 35 points on the lunar surface, with a view to rectify the determination of the equator and first meridian. These include 4 previously measured less correctly by Lohrmann and 4 by Mädler.

Mars.—Five drawings, taken in June and Aug. 1875, by Holden, with the 26-inch Washington achromatic, power 400, have been presented to the

Royal Astronomical Society. Some small white projections from the S. limb, occasionally seen, were not sufficiently steady for delineation.

The Minor Planets.—Since our last notice, No. 157 has been discovered by Borelly, at Marseilles, Dec. 1, 1875; No. 158 by Knorre, at Berlin, Jan. 4, 1876; No. 159 by Paul Henry, at Paris, Jan. 26; No. 160, in America, Feb. 25. Hilda, No. 153, is found to have a very long period of 7·85 years, and to approach Jupiter as nearly as 0·564 of the earth's mean distance from the sun.

Jupiter.—Eleven drawings, taken with the same telescope and observer as those of Mars, during June and July 1875, have been presented to the Royal Astronomical Society. That body has appointed a committee, consisting of Huggins, Knobel, Lord Lindsay, Lohse, Ranyard, Lord Rosse, Terby, and Webb, to obtain as extensive a series as possible of observations and drawings of this planet. A mean of the measures of Struve at Dorpat and Engelmann at Leipzig gives for the respective diameters of the satellites in seconds and English miles (for solar parallax 8″·875) I. 1″·048; 2,435.—II. 0″·911; 2,115.—III. 1″·513; 3,515.—IV. 1″·278; 2,970.

Neptune.—Professor Newcomb finds, with the Washington achromatic, the orbit of the satellite sensibly circular; inclination 121·°7. No trace of another satellite. Mass of primary, $\frac{1}{19,400}$.

Comets.—No new comet appeared in 1875; but the return of Encke's was closely observed. Von Asten finds in it an undoubted acceleration, not due to a resisting medium, but, as Bessel thought, to the phenomena of "outstreaming" from the nucleus. Ranyard has pointed out the evidence of a duplex structure in the comet of Coggia, 1874. With observed, July 8, a remarkable oscillatory motion in the fan-shaped jet. Schmidt assigns to its nucleus a mean diameter of 290 miles.

Double and Variable Stars.—The companion of Sirius has been repeatedly measured at Washington since 1873.—40 *Eridani.* This is an interesting and suggestive group. It is noteworthy in the first place for the large proper motion of the *lucida*, amounting, according to Mädler, to − 2″·188 in R.A., and − 3″·470 in Decl. Then for its physical connection, as proved by community of spatial displacement, with a 9·5 mag. companion, which nevertheless has kept its relative position since 1783: then for the binary character of the companion, a pair in undoubted motion: and finally for its possible connection with two other small stars in the vicinity. These were measured by Winnecke in 1864 at 75″·85 and 89″·45; and fresh measures during this winter may prove decisive, as those distances, on the supposition of mere optical juxtaposition, would be respectively varied to 41″·1 and 107″·3.—44 *Boötis.* Dr. Doberck at Markree has found the period of this star 261·12 years, with a semi-axis major of 3″·093. The same computer gives respective periods of 1578·33 and 349·1 years to ζ *Aquarii*, and 36 *Andromedæ.* For η *Cassiopeiæ* he gives 222·435 years, with a semi-axis more than twice as great as that of the orbit of Neptune. For the very difficult pair ω *Leonis* he finds a period of 107·62 years.—γ *Coronæ Australis* has, according to Schiaparelli, a period of 55·582 years. Burnham has found that his pair β *Leporis* is in very rapid motion, 12° *per annum.* —μ *Cassiopeiæ.* It appears from Smyth's "Cycle" that there was in his time a 6 mag. star just 18′ S. of μ, of a decided though not deep red. This

was not observed by Piazzi, and cannot now be found: probably a variable of long or irregular period.—The period of *Algol*, which had been diminishing since 1782, at first slowly, and then more rapidly, after remaining stationary for a time, seems to be slowly decreasing again. There is a suspicion of variation in its *comes*, first discovered by Schröter, Oct. 12, 1787, who found it sometimes quite invisible in the finest weather.—A ruby star, Schjellerup 238, near 4 *Capricorni*, considered by Sir J. Herschel perhaps the finest of that class, has been noticed by Gore in the Punjaub to be variable by at least two magnitudes.—Schmidt finds the period of Hind's red star R *Leporis* 438 d. 230 increase, 208 decreasing: he supposes the colour to be diminishing in intensity (?). This star, the most beautiful of its kind visible in England, escaped the notice of Sir J. Herschel.—The Astronomer Royal has announced that a new star-catalogue is in preparation, from the Greenwich observations of eight years. Stellar spectroscopy has been vigorously prosecuted there, especially with respect to the approach or recession of stars, as alleged by Huggins. The Greenwich observations were at first discordant, but had grown more consistent and confirmatory of Huggins's results; the obstacles, however, were extremely great, especially from the difficulty of obtaining true plane surfaces.—The lamented D'Arrest published in the "Astronomische Nachrichten," a little before his death, a continuation of his most interesting spectroscopic examination of about 11,000 stars. He verifies Secchi's fourfold classification, and his last series consists chiefly of the third type, which he finds on the whole strikingly accordant in character. The want of agreement in many cases between ocular and spectroscopic estimates of colour deserves attention.—*The Pleiades.* Wolf, at Paris, has measured 53 stars in this group, and detects 499 within $135' \times 90'$ around η *Tauri*. *Merope* and *Atlas*, with some of the smaller stars, he considers decidedly variable, and probably *Maia*.—The singular nebula near *Merope* has been watched by Wolf and Stephan, who consider its variability confirmed, as it was seen by the former in March 1874, but missed by both from Nov. 1874 to the end of Feb. 1875.—Tempel gives its dimensions $35' \times 20'$. (It has been feebly visible during the recent winter.)—The *Ring Nebula* in Lyra has been carefully examined and measured by Holden with a power of 400 on the 26-inch object-glass of the Washington observatory, and a drawing sent to the Royal Astronomical Society. Nothing new seems to have been discovered. Some brighter patches were seen on the annulus, but similar ones had been discovered and found variable by Schröter and Harding in 1797 and 1798. A minute star was also seen by Holden, near the centre, which had been noticed by Secchi and Schultz and at one time by Von Hahn, with a 20-foot reflector, in the beginning of this century, but was missed by D'Arrest. Sir W. Herschel (and Chacornac) supposed the annulus resolvable. Lord Rosse's nebulous wisps, extending outwards in all directions, were not seen at Washington, and the ends of the minor axis were reversed in sharpness at the two observatories. —30 *Doradûs*. Burton, at Rodriguez, with 12-in. silvered mirror, found some but not very essential variation from Herschel's drawing. Its spectrum was strong and continuous, crossed by a bright line. The nebula round η *Argûs* he thinks not much changed. Ellery, at Melbourne, with a telescope much more differing from that of Herschel, finds decided and pro-

gressive change in both nebulæ, most remarkably in that of η *Argûs*; the smaller class of nebulæ are also constantly changing from year to year, giving new life to such observations. The bright yellow line and occasional red line seen in the spectrum of η *Argûs* for five or six weeks, four or five years ago, have never been since seen.—The definition of the Melbourne reflector has been wonderfully improved by better adjustment, and it now seems likely to answer every expectation.—The great Paris reflector is of the Newtonian, not, as was stated in our last number, of the Lemairean or "front view" construction.

BOTANY AND VEGETABLE PHYSIOLOGY.

The Formation of Starch in Chlorophyll-grains (in the green parts of plants), under the action of light, was made out by Sachs ten or twelve years ago, and has been confirmed, and the conditions recently studied in various ways by Famintzin, Kraus, and Godlewski. It is maintained, says "Silliman's American Journal," if we rightly understand, that this starch is directly formed from carbonic acid under the action of light, *i.e.* is a primary product of assimilation. Böhm, of Vienna, who contested this, but whose view according to Sachs "has already been sufficiently refuted," has returned to the subject, with new observations and experiments, and now asserts, upon reasons stated, that "The starch appearing in the seed-leaves of plantlets of cress radish and flax, is not a direct assimilation-product, formed by the immediate decomposition of carbonic acid, but a transformation-product from a reserve of nutriment already present." One would surely expect that the primary product of assimilation of carbonic acid and water would be an organisable plasma from which the starch-grains in question are constituted, not an organic structure such as a starch-grain is. It is not clear that there is much real difference between the two views, at least between those of Sachs and Böhm. For since starch, as the former explains, is formed and reformed in various parts of the plant and away from light and from chlorophyll, and in the leaf is allowed to be only one of the products of assimilation, Sachs' assertion that starch in chlorophyll-grains is "a product of assimilation," and Böhm's that it is not "a direct assimilation-product," are by no means in necessary contradiction. Nor is there apparent reason for supposing that a starch-grain in a potato-leaf is differently originated from one in a potato, except that the former is constructed of new-formed material.

The Plants of Guadaloupe Island.—At a recent meeting of the American Academy of Arts and Sciences, Mr. S. Watson presented a paper on a collection of plants recently made by Dr. E. Palmer, in Guadaloupe Island, off Lower California. It was found to contain 119 species, including twenty-one belonging to the higher cryptogamic orders, besides a dozen of probably recent introduction. The number of new species is twenty-two, with two new genera, almost all nearly allied to Californian species and genera. Of those before known, all are Californian, and most have a wide range through that State. The flora of Mexico is scarcely represented, but on the other hand some fresh indications are found of a connection between our western flora and that of South America.

Hydrostatic Pressure in Vegetable Cells.—In a recent number of the "Botanische Zeitung," there is an abstract of a communication made by Professor Pfeffer to the botanical section of the Association of German Naturalists and Physicians, at Graz, 1875, on the subject of the origin of high hydrostatic pressure in vegetable cells. This pressure, amounting sometimes to several atmospheres, even where there is only slight concentration of the fluid contents of the cells, led him, on theoretical grounds, to refer it to the molecular condition of the primordial utricle. This conclusion was confirmed by experiment. With contraction of the molecular interspaces resistance to filtration increases, and likewise the pressure which is brought about endosmotically.

How long do Seeds preserve their Vitality?—This question, which has been often answered before, has been taken up by Herr Hoffmann, who, in a series of papers in the "Botanische Zeitung," gives his view of the matter, and describes experiments with *löss*, a diluvial earth found in the valley of the Rhine. When the railroad station Monsheim (at Worms) was built, the earth was dug away to a depth of twelve feet. Some of the *löss* was taken with necessary precautions, and securely sealed until the following spring (1865). In May twenty-four flower pots were half-filled with manure which had been heated in order to destroy any seeds present, and on this substratum some of the *löss* was placed, leaving an air-space above, of two inches, and each pot was covered by a glass disc which had a bit of wood under one edge to allow access of air. The surface of the *löss* soon had plenty of ferns and mosses, just like those which are so abundant in all greenhouses. A few phænogamic plants came up; four which could not be determined accurately were supposed to be *Vaccinium myrtillus*, a second, a *Chrysanthemum Leucanthemum;* afterwards a third came up, a *Galium*, and finally an *Equisetum*. A second series of experiments, conducted with greater care to exclude all waifs, gave wholly negative results. Some moulds, a coat of moss, and a single grass, *Festuca pratensis*, were the only plants within the bell-jars.

The Origin of Fairy-rings.—In "Grevillea" for March it is stated that in a recent communication to the Linnean Society Dr. J. H. Gilbert draws attention to the fact that, according to published analyses of various fungi, generally from one-fourth to one-third of their dry substance consists of nitrogenous matters. In fact, fungi would appear to be among the most highly nitrogenous of plants, and to be also very rich in potass. Yet the fungi have developed in "fairy-rings" only on the plots *poorest* in nitrogen and potass in such conditions as to be available to most other plants. They flourish strikingly on two plots only, in neither of which either nitrogen or potass is applied as manure, on which the development of grasses is extremely restricted, and their limited growth is due to a deficient available supply of nitrogen, or of potass, or of both, and, where the completion of the Leguminosæ is also weak, in the absence of a more liberal supply of potass. The questions obviously arise whether the greater prevalence of fungi under such conditions be due to the manurial conditions themselves being directly favourable for their growth, or whether other plants—especially grasses—growing so sluggishly under such conditions, the plants of the lower orders are the better able to overcome the competition and to

assert themselves. On this point the further questions arise, whether the fungi prevail simply in virtue of the absence of adverse and vigorous competition, or whether to a greater or less extent as parasites, and so at the expense of the sluggish underground growth of the plants in association with them; or, lastly, have these plants the power of assimilating nitrogen in some form from the atmosphere, or in some form or condition of distribution within the soil not available (at least when in competition) to the plants growing in association with them.

Germination of the Spores of "Hemileia Vastatrix."—The germination of this curious fungus has not as yet been observed in Europe; but Dr. G. H. K. Thwaites, of Ceylon, has given the results of his experiments on germination. He says that it is not difficult to induce germination. Mature spores removed from a diseased leaf, and laid upon charcoal, kept constantly moist, commence to germinate in a few days. This process consists in the spore becoming somewhat enlarged, and its contents converted into one or more globose translucent masses. From each of these a filament is developed, which grows very rapidly, and becomes more or less branched. At the termination of some of these branches secondary spores are produced in the form of radiating necklace-shaped strings of little spherical bodies of uniform size, and this form closely resembles the fructification of an *Aspergillus*. Another observer in Ceylon (Mr. Abbay) has seen another form of secondary spores arranged in simple rows of spherical bodies—a good deal larger than those radiately arranged, but still exceedingly minute. These inconceivably numerous secondary spores may be easily transported by the slightest breath of air from place to place, and from plantation to plantation. Messrs. Berkeley and Broome have intimated that this fungus seems to hold an intermediate place between Uredines and Moulds. The germination, as well as structure of the species, is thus seen to be very unique and interesting.—"Grevillea," March 1876.

"The Garden" Botanical Newspaper now issues with each weekly number a full-page coloured plate of some interesting specimen of plant. We have seen two of the representations, and can speak in the highest terms of the efforts of the artist.

Emissive Power of Leaves.—"Silliman's American Journal" states that M. Maquenne, on comparing the quantity of water evaporated by a cultivated soil during vegetation with that furnished by the rain, finds in general an excess in favour of the former. May not this excess be caused by the dew deposition at night on the plants? When the dew is measured by a pluviometer the results are much too small. The leaves condense far more than surrounding bodies, and their temperature may fall six or eight degrees below the air, showing that their emissive power is much greater than that of the metal surfaces of the pluviometer. To determine the emissive power of leaves, a Leslie cube was employed; one of its faces was blackened, another covered with leaves, and the two surfaces turned successively to the pile. The temperature of the water did not exceed 40°, to avoid injuring the leaves. The deflections were measured by a mirror and scale, and a twentieth of a degree was easily observed. On trying several kinds of leaves it appeared that their emissive power did not differ greatly, was the same on both sides, and had an average value of 94, that of lamp-

black being 100. To measure the absorbent power, a thermopile was formed of a thin sheet of copper riveted to a steel spring. One face was covered with lampblack, the other with the leaf to be examined. Exposing the two surfaces in turn to the radiation of a metallic blackened box heated by steam, and waiting until the galvanometer needle came to rest, the ratio of the deviations gave the absorbent power of the leaf. From the results it appeared that the absorbent power is sensibly equal to the emissive power, and consequently the amount of dew deposited on plants should be determined by pluviometers painted black.

The Functions of the Stomata in Plants.—This subject has been recently investigated by Mr. T. Meehan, who has presented a paper on it to the Academy of Sciences of Philadelphia. From this it seems that the current ideas as to the functions of those structures are somewhat erroneous. Mr. Meehan exhibited a leaf taken from a small tree of *Acer pseudo-platanus* —the common sycamore maple—which had assumed an inverted position. The tree was three years old from seed, and all the leaves were of that character. The young leaves first appeared in their normal condition, but as the petiole lengthened, the leaf blade bent under, so that the under surface was exposed to the full sunlight, and under the petiole above. He also exhibited a young oak from an acorn sown in the spring, and which he believed to be *Quercus Catesbæi*, and in which all the leaves were vertical, and not with their surfaces on a plane with the horizon, as is the case with all other seeding oaks and American trees. He said it was possible this position of the leaves was not continued with the increased age of the tree, or it would have been observed and placed on record. Of several hundred young plants, all had the same character. The facts were simple in themselves, but had great interest to the student in vegetable physiology. The stomata were usually on the under side of the leaf, and believed to be there of a necessity. Our leading botanical textbook taught that stomata were breathing pores, and could not carry on their essential operations when exposed to direct sunlight; and the same high authority had suggested that if leaves of this character could be inverted, and forced to remain in this condition, the plant would inevitably die. The maple did not die, but had been during all its existence as healthy as others of the same species growing near it. A large number of the proteaceous and myrtaceous plants of South Africa and Australia, and of which the now famous *Eucalyptus globulus* is a familiar example, had their leaves vertical, as in this oak. This had been accounted for by the statement that these leaves had stomata on both surfaces of the leaf, and the effort of these stomata to face the earth had of course resulted in an even balance of power, in which neither side had any advantage. The stomata on each side of the leaf had to face the horizon. Supposing this might account for the position of these oak leaves, they, as well as the maple, had been examined microscopically by Dr. Hunt, of the Academy, and found to have stomata only on one—the normal side. He thought it safe to conclude, from these facts, that the accepted theories of the relation of stomata to light required some modification.

Extraordinary Growth of Vallisneria spiralis.—Mr. A. W. Bennett, M.A.,B.Sc., asks the editor of the "Journal of Botany": Can any of the

readers confirm his observation of the extraordinarily rapid growth of the flower-stalk of this plant? It was first observed in his aquarium about 10 A.M. on July 19, and measured 26 inches, this being almost certainly the growth of the previous forty-eight hours. At 10 A.M. on July 20, it measured 38 inches, or had grown 12 inches in twenty-four hours. At 4 P.M. on the same day the length was 41 inches; at 10 A.M. the next morning 42¼; and 10 A.M. on the 22nd, 43 inches, its ultimate length. From the time when first observed—the 19th—till the 31st, when he left home, the flower, a female one, remained open without any apparent change, not being fertilised during that time, as no male flower made its appearance. There was no coiling or uncoiling of the flower-stalk during the whole time; only a slight waviness. Since he published this statement, observations of a similar character have been made by Mr. W. Reeves, F.R.M.S., the Under-Secretary of the Royal Microscopical Society, and by various botanists, all showing the very rapid-growing powers of this plant. We do not know whether there is any male plant in this country.

Colossal Redwood Trees: Sequoia sempervivens.—R. E. C. S., writing in the "American Naturalist," says that, at a recent meeting of the California Academy of Sciences, Dr. A. W. Saxe made a preliminary report on a grove of colossal redwood trees that have been discovered on the course of the San Lorenzo, which takes its rise near Saratoga, in Santa Clara County, and debouches into the Bay of Monterey, at Santa Cruz. The trees are in a forest around the head-waters of the stream. One of them eclipses all that have been discovered on the Pacific Coast. Its circumference as high as a man can reach, standing and passing a tape line around, is a few inches less than 150 feet. This is beyond the measurement of any of the *Sequoias* (*gigantea*) in the Calaveras Grove. The height is estimated at 160 feet, and a part of the top lying on the ground riven off by lightning, or a tornado, is over 100 feet in length. The other trees in the vicinity are not as large, but all are of immense girth. Dr. Saxe promised to get information more in detail from the President of a flume (?) company in that section. This region has but recently been explored, and what other marvels of vegetation it contains remains to be seen. The stumps of redwood trees, of immense proportions, have been reported, from time to time, to the Academy, by explorers in the Mt. Diable range along the hills back of Oaklands; but now we are likely to have further discoveries of these majestic conifers in their glory, height, diameter, and foliage.

CHEMISTRY.

On Parabin, a new Carbohydrate.—Herr Reichardt has prepared from both the tissue of beets and of carrots, a new carbohydrate, which, on account of its close resemblance to Scheibler's arabinic acid, he calls *pararabyn*. The root is rasped, the pulp pressed out, treated with water and alcohol to remove everything soluble, digested for several hours with a one-per-cent. solution of hydrochloric acid, heated to boiling, and the liquid strained off. Alcohol throws down a gelatinous precipitate, which after washing with

alcohol and drying, forms a friable white powder, swelling up in water and dissolving on the addition of an acid when heated. Alkalies precipitate it again, and it gives no sugar by the action of sulphuric acid. Its formula is $C_{12}H_{22}O_{11}$. It differs from arabinic acid by its neutral reaction and its chemical indifference; by its yielding no sugar; by its solubility in acids and precipitation by alkalies instead of the reverse. If, however, it is acted on for a long time by an alkali, or if it be warmed in contact with it, it is converted into arabinic acid. Quantitative experiments showed that 38·5 per cent. of the beet pulp was arabinic acid, 54·0 per cent. pararabin, and 7·5 per cent cellulose.

Ethyl Alcohol in Plants.—The occurrence of ethyl alcohol in the unfermented juices of plants is rendered probable by the fact that these juices contain not only acetic acid, its oxidation product, but also its homologues, methyl, hezyl, and octyl alcohols. Herr Gutzeit has made, in Geuther's laboratory, a somewhat extended investigation of this subject, using for the purpose *Heracleum giganteum*, *Pastinaca sativa*, and *Anthriscus cerefolium*. Six and a quarter kilograms of the fruit of the first plant being distilled with 18 kilograms of water, gave 12 kilograms of a distillate, from which the oil on the surface was separated by means of a syphon. The residue on distillation gave 12 to 15 grams of volatile liquids, which, after rectification, boiled between 72° and 77°, and consisted of methyl and ethyl alcohols, the latter constituting two-thirds. By examining the fruit at various stages of growth, the author infers that as ripening approaches, the ethyl compounds gradually diminish, being converted into more condensed bodies; while the methyl compounds remain constant. The other plants mentioned gave a similar result.

Circumstances which affect the Crystallization of Sugar.—M. Durin has a paper on this subject in the "Comptes Rendus." In the commercial analysis of sugars it is not sufficient to determine the mere quantity of pure sugar present. It is needful also to know the amount of certain foreign bodies which have the power of interfering with the crystallization of sugar in the process of refining. Salts hinder crystallization to the extent of four or five times their own weight. Glucose has also been considered as obstructive to the extent of twice its weight. The author finds that crystallizable salts do not interfere with the crystallization of the sugar, but that the formation of treacle is due to organic matters and deliquescent salts present in the juice of the cane and the beet. Nevertheless, the assumption that each part of saline matter will hinder the crystallization of four parts of sugar, though it is founded on a theoretical error, gives results satisfactory in industrial practice. Glucose, contrary to the usual opinion, does not interfere with crystallization.

A Royal Society's Medal to Mr. Crookes, F.R.S.—We are sure those of our readers who are at all familiar with chemical or physical research will be delighted to hear that a royal medal has been awarded this year to Mr. Wm. Crookes, F.R.S., for his various chemical and physical researches, more especially for his discovery of Thallium, his investigation of its compounds and determination of its atomic weight, and for his discovery of the repulsion referable to radiation. Whatever views we may hold as to Mr. Crooke's opinions on spiritualistic subjects, we can have no hesitation in expressing

our extreme gratification at this mark of appreciation being shown him by the Royal Society.

Detection of Bromoform in Commercial Bromine.—It is stated that in titering, by means of potassium iodide, a solution of bromine in water, Herr Reymann observed that the result obtained was too low, and that the liquid possessed a peculiar odour recalling that of chloroform. Further investigation showed the bromine to be mixed with at least 10 per cent. of a substance boiling between 80° and 165°, the principal part of which consists of bromoform. It is readily detected by the influence it has in lessening the solubility of bromine in water as well as by its odour, which is most readily perceived when the bromine is agitated with a solution of potassium iodide and the whole decolorized by sodium thiosulphate.

A New Acid and Oxide of Uranium.—In a preliminary notice which Mr. T. Fairley, F.R.S.E., has sent to the "Chemical News," he explains the action of hydrogen dioxide on salts of uranium. He says it is remarkable, and takes place even in presence of much free acid. On mixing solutions of uranic nitrate and hydrogen dioxide, a yellowish white precipitate is obtained, which, when washed and dried at 100° C, retains one atom of water, and gives numbers agreeing well with the empirical formula NO_2,H_2O. Its real formula is no doubt some multiple of this formula. This oxide is, by its decomposition with alkaline hydrates, shown to be a compound of a higher oxide of uranium, NO_3, with uranic oxide, N_2O_3. The sodium, potassium, and ammonium salts of this acid have been prepared. The sodium salt is readily obtained in crystals by mixing strong solutions of uranic salt with excess of hydrogen dioxide solution (5 per cent.), and then adding strong sodium hydrate solution in quantity sufficient to dissolve the precipitate. If weaker solutions be used, the addition of a little alcohol will separate the sodium salts in crystalline plates. The full analyses of these compounds he will publish shortly. By means of hydrogen dioxide uraninm may be separated from all other metals, and in acetic solutions either uranic salt or hydrogen dioxide may be used to titrate each other using potassium ferrocyanide as indicator.

GEOLOGY.

Flint Implements from the Brixham Cavern.—We have received a series of very admirable photographs from Mr. N. Whitley, done by himself, o specimens of flint weapons which he alleges are taken from those to be seen at the Christy Museum. Of the more than thirty examples in the photographs there are hardly more than two which strike us as being of human workmanship. We have carefully examined them, and have submitted the photographs to the opinion of an eminent geologist, who thoroughly agrees with us in regarding these photographs, with one or two exceptions, as those of purely natural specimens of flint.

Oolitic Brachiopoda.—We learn from the "Geological Magazine" that Mr. J. F. Walker, M.A., F.G.S., exhibited at a late meeting of the Yorkshire Naturalists' Club the following species of Brachiopoda which occur on the Continent, but are scarcely known as British species, viz. *Terebratula*

bisuffarcinata, Schlot., and *Rhyn. Thurmanni*, Voltz., from the lower calcareous grit of Filey, Yorkshire coast; *Waldheimia umbonella*, Lamarck, from the Kelloway rock of Scarborough; *Terebratula Eudesii*, Oppel, and *Terebratula ventricosa*, Yieten, from the inferior oolite of Cheltenham; and *Terebratula Ferryi*, Des., from the inferior oolite of Dorsetshire.

Carboniferous Conifers.—Professor J. W. Dawson writes as follows from Montreal to "Silliman's American Journal":—Little attention seems as yet to have been given to the remains of coniferous trees found in the carboniferous rocks of the United States. In Nova Scotia several species are known, and are to some extent characteristic of definite horizons. In the carboniferous sandstones of the United States such remains seem to be frequent; but I have seen no detailed account of them, and the only well characterized specimens which have come into my hands are portions of two trees from Ohio kindly sent to me by Dr. Newberry, and a very finely preserved fragment from Mazon, Grundy Co., Illinois. Both of these show the characteristic structure of *Dadoxylon* (Unger), *Araucaroxylon* of Schimper's recent work, and are closely allied to *D. materiarium*, the common species of the upper and middle coal formations of Nova Scotia. The specimen from Illinois is probably identical with that species. One of these from Ohio presents some peculiarities in the structure of the medullary rays, which may indicate a distinct species. The subject is deserving of the attention of microscopists in the coal districts, as there can be little doubt that several interesting species remain to be discovered, and other kinds of structures; as for example the curious *Dictyoxylon* of Williamson, found also in Nova Scotia, would very possibly reward patient slicing of trunks showing structure. The Devonian has also treasures of the same kind. In the United States it has already afforded *Dadoxylon Halli* from New York, and *D. Newberryi* from Ohio, besides the curious *Ormoxylon Erianium*. No doubt other species remain to be discovered, especially in the Upper and Middle Devonian. The writer of this note would willingly correspond with anyone engaged in such researches, as he has now under examination a number of specimens from different parts of British America, and would be glad to have opportunities of comparison with those of the United States.

A Query as to the Bones of the Hadrosaurus: Clavicles or Ischia?—In the Reports just issued of the Academy of Nat. Science, Philadelphia, it is said that Professor B. Waterhouse Hawkins referred to his remarks of last summer regarding the position of the so-called clavicles of *Hadrosaurus*. Having drawn a figure of the skeleton, he explained the impossibility of disposing of those bones in the position of clavicles. A comparison of the skeleton of *Hadrosaurus* with that of the ostrich was made, and the conclusion drawn that the ischiatic position assigned to the bones in question in the restoration made for the Smithsonian Institution was unwarranted. Prof. Cope stated that he had lately obtained a metatarsal bone of *Lælaps*, which confirmed the views of Professor Hawkins, as expressed in his restoration of that animal made during his engagement at the Central Park, New York.

Nomenclature of the Drift.—In reference to this subject Mr. G. H. Kinahan has written a letter to the "Geological Magazine," in which he gives an account of the Irish gravels. He says that as to those, they have never been systematically examined or classed. "We have

different gravels—1st, under the 25-feet contour-line; 2nd, under the 110-feet contour-line; and 3rd, under the 350-feet contour-line—all more or less containing marine shells. Although these gravels are of distinct ages, yet the fossils collected from them have been lumped together. Then older than the Esker gravels (under the 350-feet contour-line), there are gravels at about the following respective heights—550 feet, 750 feet, and 1,200 feet, some of which contain fossils; and although these gravels must be much older than the three groups first named, yet their fossils have been all classed together. I remember hearing my brother, the late Dr. Kinahan, remark that the group of fossils from the gravels at Bohernabreena (about 200 feet) were distinct from the group found in the gravels at Howth and the coast to the northward (under 100 feet). In no place in Ireland have I seen gravels belonging to the first three groups (the third being the so-called 'middle gravels') under normal glacial drift, although I have found gravels belonging to all the others so situated."

Scotch Fossil Trees.—We learn that six trunks of large trees have been obtained at Craigleith quarry since 1826. The largest, 36 feet long and 12 to 14 feet in girth, has been taken to the British Museum, and is to be set up erect there. Another is nearly 30 feet long, and has been removed to the Botanic Gardens. The trees were conifers. The surface of each is bituminous coal, varying from one-twentieth of an inch in thickness to two inches. The trunks inside of this coaly exterior consist of carbonate of lime, carbonate of magnesia, carbonate of iron and free carbon in varying proportions. The coaly exterior is attributed, by Sir R. Christison, to bitumen passing to the surface as the destructive distillation of the wood was going on within.

MEDICAL SCIENCE.

The Brain in Man and the Lower Animals.—In the "Proceedings of the Royal Society" (No. 163) appears a paper by Prof. Marshall, F.R.S., in which the author says:—"1. I desire to communicate to the Royal Society the fact that I have, by severing the cerebral hemispheres in certain definite directions in man, and also in some of the higher vertebrata, and by then weighing the separated portions, not only arrived at some interesting and important results as to the relative size of those portions in different animals and in man, but I am enabled to state that this method, applied to the brains of individuals of different race, sex, age, education, and occupation, seems likely to furnish a means of investigating individual peculiarities in the human cerebrum. I propose shortly to communicate my results to the Society. 2. I have likewise made numerous observations on the convolutions of the human brain, with the view of explaining their symmetry in certain regions, and their asymmetry in others. In endeavouring to trace more particularly the causes of the asymmetry of the convolutions which prevails in man, I have been led to believe that some, at least, of this is due to the right-handedness of man. I find, on studying a large number of human cerebra, that there are stronger evidences of *essential* asymmetry, as distinguished from

what I would term *non-essential* asymmetry, in the immediate neighbourhood of the left fissure of Rolando, and next to this part in the right parietal lobule. There are certain secondary essential asymmetrical conditions which may be pointed out, and besides this many non-essential and very variable ones. Evidence can be given in support of these propositions from the examinations of fœtal brains and the brains of idiots, the former of which exhibit a remarkable, *early*, and special tendency to deviations in symmetry in the neighbourhood of the left fissure of Rolando.

Chloral a Substitute for Spirits in Preserving Anatomical Specimens.—The "Philadelphia American Times" contains an article by Dr. W. W. Keen upon the anatomical, pathological, and surgical uses of chloral, in which he recommends this substance very strongly for the preservation of objects of comparative anatomy and natural history. It is used by injection into the blood-vessels, or by immersion, and in his opinion it is likely to supersede many of the preparations now in use. Its special advantage is that the colour of the object is preserved perfectly, and all the parts have a natural consistency, while there is nothing either poisonous or corrosive to affect the general health of the experimenter or to injure instruments. For preserving a subject for dissection, half a pound of chloral will suffice, at a cost of a dollar or less. A solution for preserving specimens of natural history of ten or twelve grains to the ounce of water is quite sufficient, is much cheaper than alcohol, and the bottles instead of being hermetically sealed are closed by glass stoppers, or even ordinary corks. Dr. Keen has thus kept pus from various substances, and diseased growths of various kinds of other specimens, for months, and found no change whatever in their character. Chloral is extremely antagonistic to fungi and infusoria, a very weak solution of it killing them instantly. The deodorizing as well as the antiseptic properties of chloral are equal, in Dr. Keen's opinion, to those of any substance now known.

The Alkaloids contained in the Aconites.—Dr. Wright, of St. Mary's Hospital, has been investigating this important subject. In a paper read before the Chemical Society he says that the material for this investigation was furnished by Mr. T. B. Groves, who prepared it in the manner described in the "Year-Book of Pharmacy," 1873 and 1874. The authors describe several bases which they have isolated from this mixture, one of which, having the composition $C_{31}H_{45}NO_{10}$, appears to be comparatively inert, whilst another, of the composition $C_{33}H_{45}NO_4$, has a powerful physiological action. A pseudaconitine, $C_{36}H_{49}NO_{11}$, which forms indistinctly crystalline masses, was also obtained. The authors consider that either the roots of the aconites contain several alkaloids capable of crystallising or of giving crystalline salts, or else that the alkaloid originally present is readily alterable and gives rise to numerous altered products during the processes of extraction and purification.

An American View of British Rivers.—The New York "Graphic" tells us some very extraordinary facts!! It says that "the pollution of rivers is a topic that taxes the attention of British sanitarians to the utmost. The extent of the difficulty is certainly alarming. Nor is it caused solely by the sewage of cities and towns, bad as that is. The rivers are lined with manufactories of all sorts, chemical works, machine shops, and dye-houses, all of

which pour their poisonous refuse into the rivers. And so great is the pollution of the water in some instances that when a light was applied to some of it dipped from the river at Bradford it actually burned. A man who accidentally tumbled into a river and swallowed a mouthful of water died of the effects. The effluvia that rises from the Clyde produces sickness in summer time, and the Mersey emits an unbearable stench. The water of the Bourne is yellow as ochre and thick as glue, and the horse that drinks it dies. And all the rivers are more or less affected in the same way, and the fish that survive in some of the streams are so unwholesome that they are unfit for food, if not dangerous."

Respiration in Insects.—In a paper recently published (in the "Central-Blatt für Agri. Chem." Heft 6) Herr O. Bütschli records some experiments which he conducted on the common cockroach (*Blatta orientalis*). From these we learn that the liberation of carbonic acid was found to be directly proportional to the temperature.

Variations in the Strength of a Muscle.—Professor F. E. Nipher publishes a valuable paper on this subject in "Silliman's American Journal." The conclusions drawn by the author are :—1. After the relation of the strength of a muscle to the dynamical work of exhaustion has been determined, the strength at any time will be most accurately determined by measuring the dynamical work of exhaustion. On days when from any cause the muscles are temporarily weak, the strength as determined by the dynamometer, and the work of exhaustion with very heavy weights, is less. In exhaustion with lighter weights, however (5. kgr.), the exercise of the first part of the experiment appears to invigorate the muscle, and the influence of temporary weakness, due to errors in diet, or lack of exercise, or the oppressive atmosphere of the room, is eliminated. 2. The co-efficient of muscular power per square centimeter of section of the muscle, is a quantity which varies greatly with different muscles, and with the same muscle at different times; or, in other words, the work which a muscle can perform depends not only upon its size but also upon its quality.

The Body rendered Luminous by Phosphuretted Hydrogen.—The editors of "Silliman's American Journal" state that Dr. Geo. Maclean, of Princeton, in a private communication to them, says:—"Several years ago, after spending a portion of the day in experimenting with this gas (PH_3), prepared from phosphorus and solution of potash, on retiring to bed I found my body quite luminous, from a glow like that of phosphorus exposure to the air. Either some of the gas escaping combustion, or the product of its burning, must have been absorbed into the system, and the phosphorus afterwards separated at the surface have there undergone eremecausis. I was conscious of no feeling that could be attributed to it, nor was my health apparently in any way affected by it." Probably a repetition of the trial would reproduce the results observed by Dr. Maclean, which have apparently escaped notice heretofore because unsought.

Unipolar Electric Excitement of the Nerves.—The "Chemical News," giving an account of a paper lately read before the French Academy, says that M. A. Chaveau states that for every subject whose nerves are in a perfect physiological state there is an electric value — commonly very weak, sometimes moderate, but rarely very high—which gives to the

two poles the same degree of activity in the case of unipolar excitement of the motor nervous bundles. The contractions produced by positive and negative excitement with this typical intensity of the current are equal, both in magnitude and duration. Below this intensity equal currents produce unequal effects with the two poles, the activity of the negative pole being more considerable. When tetanisation is produced by feeble currents, it is never with the positive pole upon the nerve. Above the typical value of the intensity of the current the inequality appears in the opposite direction.

METALLURGY.

Samarskite in America.—At a recent meeting of the Academy of Philadelphia, Mr. Joseph Willcox called attention to a mineral specimen (samarskite) presented by him, which was found at a locality discovered by him recently among the mountains in Mitchell Co., North Carolina. Excepting in North Carolina, this rare mineral is only found in the Ural Mountains in Asia. According to Dana's "Mineralogy," the largest specimens found at the latter locality are only as large as hazel-nuts; but Mr. Willcox said he obtained a specimen in North Carolina that weighs more than twenty pounds. It was associated with decomposed feldspar.

The Association of the Native Platinum of the Urals.—A writer in the "Geological Magazine," whom we take to be Professor Morris, says that M. Daubrée, in an interesting paper before the Academy of Sciences, has shown that native platinum, although obtained abundantly in the alluvial deposits of certain regions of the Ural, has been found in a Peridote (Olivine) rock, which is more or less altered into serpentine, and accompanied with diallage (a ferruginous sahlite, according to M. Des Cloiseaux), and also with chromite, which occurs abundantly, not only in separate grains, but also encrusting the grains of platinum. The platinum, which is here associated with chromate of iron, appears to be distinguished from the platinum of other deposits by the large proportion of metallic iron with which it is alloyed. It appears that platinum very rich in iron, and endowed with magnetic polarity, has not been found—at least, at present—save in company with chromate of iron.

MICROSCOPY.

The New Platyscopic Pocket Lens promises to have a very great success, for it certainly completely distances the Stanhope or Codrington in largeness and flatness of field, if not in magnifying power. It is certainly the best pocket lens that we have ever seen, and we have very carefully examined and tested it before expressing this opinion. It is a triple achromatic combination, in which the chromatic and spherical aberrations are corrected by the central lens of dense flint. This lens is nearly three times as thick as the crown-glass lenses. The interior curves are almost hemispheres. The final correction of spherical aberration is made by altering the thickness of the

dense flint glass lens. The three lenses are united by a transparent cement which has a refractive index corresponding very nearly with the glass. This prevents light being lost by reflexion from the surface of the deep curves. The new lens is manufactured by the inventor, Mr. J. Browning, the optician, of 111 Minories, E.C.

Diatomaceæ to be Obtained from Coal.—The "Monthly Microscopical Journal" says that the following method of obtaining the diatomaceæ from coal for the microscope is described by Count Castracane:—"The course to pursue is decided by the flinty nature of the diatom valves, and in order to separate them from the mixture of calcareous or organic matter with which they are found united, it is usual to put the whole into a glass test-tube with hydrochloric acid, adding chlorate of potash from time to time, keeping all slowly dissolving by heat, in order to isolate the silex, destroying the remainder. But in unburnt coal it is too difficult to dislodge the carbon, and the acids have little effect upon it. I must, however, refer to the calyination I effected by grinding up the substance, and then, collecting it in a china vessel, placed upon a stove in a glass tube, subjecting the whole to the action of the heat, while, at the same time, a slight current of oxygen crossing the tube combined with the carbon in creating acid. Experience has taught me, however, the necessity of conducting this operation at a lower temperature, in order to prevent the alkaline or earthy bases and metallic oxides, which may be amongst the ashes, from forming vitreous silicates by melting and mixing with the valves of the Diatomaceæ. It is also well to leave the glass tube, in which the fusing is going on, uncovered, in order to watch its progress. The small residue obtained through this process is to be put into a clean test-tube, adding nitric acid and hydrochloric acid and caustic potash, assisted by the heat of a lamp, to eliminate any alkaline or earthy base and every trace of metallic oxides. The last operation over, it only remains to wash rapidly with distilled water the very light dust which is left behind, letting it stand for some hours to settle, in order to be sure of not losing the smallest particle of it in pouring off the water. Those who follow this method exactly cannot fail to succeed. The object may then be mounted with Canada balsam, or in any other suitable medium; and steadily and closely watching it, under the microscope, they will not be long before they see some valves of diatoms, entire or broken."

The Leaf Glands of Saxifraga tridactylites.—The "Monthly Microscopical Journal" of Jan. says that an addition to our list of carnivorous plants is suggested by Mr. J. C. Druce, in a letter to the "Pharmaceutical Journal," in a little early spring flower found chiefly on the tops of walls, *Saxifraga tridactylites*, a plant not very distantly allied to the Droseras. Mr. Druce states that when examined under the microscope the leaves are seen to be covered with glands of a similar character, which exude a viscid secretion, in which he found a midge was entrapped and held fast when placed on the leaf. On examining a number of leaves, he found in all of them the débris of insects which had apparently perished in this manner.

Mode of looking at a Watch through a Beetle's Eye.—Dr. Whittell, of Adelaide, Australia, has a paper on this subject in the "Monthly Microscopical Journal," March, in which he gives the following instructions as to the mode of mounting the specimen:—Take a watch with a white face, take

out the front glass, and remove the hour and minute hands. Paste over the face of the watch a piece of dead-black paper with a round window cut in it, so as to leave nothing exposed but the small circle in which the seconds hand rotates. Place the watch on the front of the mirror of the microscope, and condense the light of a strong flame on the small white circle that has been left exposed. Reflect this light through the beetle's eye, previously placed on the stage, just in the same manner as if the ordinary mirror were being employed. Bring the eye into focus, and then gradually draw back the objective by means of the fine adjustment until the images of the watch hand appear. At first these will probably be dim, but by varying the inclination of the watch and careful adjustment of the light the observer will at length obtain a bright and distinct image through each lens of the eye. The nearer the watch can be brought to the stage without cutting off light from the condenser, the larger will be the image. Any power may be used from $\frac{1}{8}$ to $\frac{1}{3}$ inch, but I prefer a $\frac{4}{10}$th, with a No. 2 eye-piece. Under this power the images are sufficiently enlarged, and a good number of them are included in the field. The eye may be mounted in balsam, but I think I have obtained better results from one specially prepared and mounted in glycerine.

Microscopical Papers of the Quarter.—The following is a list of the contents of the "Monthly Microscopical Journal" for Jan., Feb., and March 1876:—

The Absorptive Glands of Carnivorous Plants. By Alfred W. Bennett, M.A., B.Sc., F.L.S., Lecturer on Botany at St. Thomas's Hospital.—Reproduction in the Mushroom Tribe. By Worthington G. Smith, F.L.S.—Avoiding the Use of the Heliostat in Micro-photography. By G. M. Giles, M.M.M.S.—Improved Method of Applying the Micro-spectroscopic Test for Blood-stains. By Jos. G. Richardson, M.D., Attending Physician to the Presbyterian Hospital; Microscropist to the Pennsylvania Hospital.—Remarks on the Foraminifera, with especial reference to their Variability of Form, illustrated by the Cristellarians. By Professor T. Rupert Jones, F.R.S., F.G.S.—The President's Address. By H. C. Sorby, F.R.S., F.L.S., F.G.S., F.Z.S., &c.—Further Notes on Frustulia Saxonica. By W. J. Hickie, M.A., St. John's College, Cambridge.—On the Characters of Spherical and Chromatic Aberration arising from Excentrical Refraction, and their relations to Chromatic Dispersion. By Dr. Royston-Pigott, M.A., F.R.S., F.C.P.S.– On Staining and Mounting Wood Sections. By M. H. Stiles.—On a Mode of Viewing the Seconds Hand of a Watch through a Beetle's Eye. By Dr. Whittell.

PHYSICS.

Utilising the Solar Rays.—A method for this purpose has been described before the French Academy by M. A. Mouchot. The author's apparatus is composed of three distinct pieces—a metallic mirror with a linear focus; a blackened boiler, the axis of which coincides with this focus; a glass enclosure, which allows the solar rays to reach the boiler, but opposes their

exit as soon as they have been transformed into heat-rays. The yield of a large solar heat-generator is greater than that of a small one. The whole apparatus is arranged so as to turn 15° hourly around an axle parallel to the axis of the earth, and to incline gradually to this axle, according to the sun's declination. An apparatus of this kind, erected at Tours, gave the following results:—In ordinary fine weather, 20 litres of water at 20°, introduced into the boiler at 8.30 a.m., produced in forty minutes steam at a pressure of 2 atmospheres, or a heat of 121° cent. This steam rapidly rose to the pressure of 5 atmospheres, a limit which it was not safe to exceed. About mid-day, with 15 litres of water in the boiler, the steam was raised in fifteen minutes from 100° degrees to 153°, a pressure of five atmospheres. Hence the author concludes that in hot and sunny climates the sun's rays may be successfully utilised as a source of mechanical power.

A Monster Battery.—A chloride of silver battery of 3,240 elements has been described by Mr. Warren De la Rue and Mr. A. W. Müller. The description has appeared in French, but it is thus translated in the "Chemical News." It is composed on the one part of 1,080 elements, each consisting of a tube of glass 15·23 c. in length, and of 2,160 elements formed of glass tubes of 12·75 c. in length only. All the tubes are 1·9 c. in diameter, and are closed with stoppers of vulcanised india-rubber, perforated with a hole near the edge to permit the introduction of a rod of amalgamated zinc, 0·48 c. in diameter, and 10·43 c. in length for the first 1,080 elements, and 7·93 c. for the remainder. At the bottom of each tube powdered chloride of silver is placed, 14·59 grms. in weight, compressed strongly with wooden rods, a flattened silver wire having been first introduced to the bottom of the tube. The silver wires are covered in their upper part, above the chloride of silver, and up to the point where they emerge from the vulcanised stopper, with leaf gutta-percha to isolate them and preserve them from the action of the sulphur in the stoppers. The electromotor force of this battery is to that of a Daniell's battery as 1·03 to 1.

A Novel and Useful Form of Bunsen Burner has been described in the "Chemical News," by President Henry Morton, Ph.D., of New Jersey, in the following terms:—"The retreat of a burner will evidently occur whenever any part of the ascending column of mixed gas and air is moving at the orifice with a velocity less than that at which the same will burn downwards. Now, in an ordinary burner, with its main tube of regular cylindrical bore, it is evident that the friction of the surface of the ascending column of mixed gases will cause that portion to move at a less velocity than the central part, and that currents of the nature of eddies will be developed. It will thus happen that while the central portion of the ascending column of gaseous mixture issues at a velocity much greater than that at which the material can burn downwards, and thus is quite free from any danger of retreating, the marginal portions of the column or jet of gas will be escaping at a rate so much less that the velocity of their combustion downwards will exceed that of their upward motion, and retreat of the flame will ensue. It is well known that to secure a jet of water, or of any other fluid whose particles shall move with equal velocities in all parts, and thus avoid currents and eddies, it is only necessary to make the orifice of efflux an aperture in a thin wall. Following out the idea above indicated, I

made a burner of a bore rather large compared with its height, and then drew in its upper edge into the form of an open-ended thimble, so contracting the orifice of escape to about two-thirds the area of the tube, and rendering this orifice practically an opening in a thin horizontal wall or plate. The results of this modification far surpassed my anticipations. A burner thus constructed gives a perfectly nonluminous flame with gas pressures varying between 1·5 and 0·1 inch of water, and with the lowest of these pressures cannot be made to retreat by the most violent handling in the way of sudden movement or waving about in the air, even when this violence is carried to the extent of extinguishing the flame altogether. Under like conditions of pressure, a burner of the ordinary construction is made to retreat by a slight draught of air, or a very moderate amount of motion."

The last Form of French Balloon is thus described in a recent paper published in the "Chemical News," on the subject of the "development of the chemical arts during the last ten years," by Professor A. W. Hofmann. Speaking of aeronautics, he says the last impulse was given, not by festivity, but by the terrors of war and the siege of Paris. The Académie des Sciences commissioned one of its members, Dupuy de Lôme, to make experiments on steering balloons, and the Government furnished the requisite means. Dupuy gave his balloon the fish shape, and, in order to render its shape stable in the wind, he fitted it with an internal secondary balloon (*ballonet*), containing more or less air, and equal in bulk to one-tenth part of the main balloon. The air could be let out of this inner balloon by valves, or driven in again by means of a bellows in the car, according to a plan which Meusnier had devised as early as 1783 to supersede the use of ballast. Dupuy's balloon was further distinguished by a very firm method of suspending the car, and by the use of a varnish impermeable to gases, and made of gelatin and tannin dissolved in pyroligneous acid. The propelling screw was not turned by a steam-engine, but by eight men in the car. The balloon, containing 3,450 cubic metres, was filled with hydrogen obtained from iron and sulphuric acid, and went up at Vincennes on February 1st, 1872, carrying fourteen persons. After a flight of two hours, it was let down at Noyon, a distance of 106 kilometres. By means of an anemometer, Dupuy was able to determine the independent speed of the balloon at 2·82 metres per second, whilst that of the wind was 16 to 17 metres, and the course of the balloon made an angle of 12° with the direction of the wind. The problem of steering had, therefore, been solved, though only to a very slight degree—sufficient for a calm, but insufficient for overcoming even moderate winds. The speed attained was slight. Both conditions of success depend on the employment of stronger sources of mechanical power, and this, again, requires an increase of its power of ascent, *i.e.* of its relative levity with an enlarged volume.

Gyratory Movements of Salts in Water.—M. M. H. Lescœur says ("Bull. de la Chimique de Paris"), that substances possessing the *epipolic* force may be arranged in two classes:—1. *Bodies Insoluble in Water.*—When once they are spread out all motion is arrested. and the movement of every other body is suspended (fixed oils, fatty bodies, &c.). 2. *Soluble or Volatile Bodies.*—The superficial layer produced instantaneously dissolves, or is volatilised. The movement is continuous. The saturation of the liquid and

ambient air causes all activity to cease (camphor, acetic acid, essential oils, &c.). As to the first cause of the motion, it is found in the reciprocal action of two fluid surfaces. It may be a phenomenon of capillarity, or of the superficial tension of the liquids.

Electro-Conductibility of Pyrites.—M. H. Duffet, in the "Comptes Rendus," contends that the conductibility of iron pyrites is a true metallic conductibility, very variable according to the physical structure of the specimen, but which, in a given crystal, is affected neither by the direction, nor the intensity, nor the duration of the current.

Common and Magnetic Iron in the Atmosphere.—It is alleged by M. G. Tissandier, who, as our readers are aware, is an eminent balloonist, that in specimens of atmospheric dust examined by him he has discovered minute particles of magnetic iron ore, which he believes to be of cosmic origin.

Influence of Forests on the Quantity of Rain.—The "Chemical News" says that the absolute quantity of moisture in the atmosphere appears to differ little at any season of the year in open grounds, and in forests. The relative moisture is, however, greater in forests, the difference being greatest in summer, and increasing with the elevation of the place above the sea-level. According to Ebermayer's view, forests increase the amount of rain only by their action upon the relative moisture of the atmosphere.

Spectroscopic Reactions of Hæmoglobin and its Derivatives.—M. C. Husson gives the following observations, according to a notice in the "Chemical News."—Hæmoglobin, on absorbing iodine, splits up into hæmatin and globulin. This fact is proved by the spectral analysis, which gives between C and D the absorption ray of hæmatin, which does not seem to be affected by the iodine. Chautard has already shown that this element has no influence upon the rays of chlorophyll. The microscope also indicates the splitting up of hæmoglobin. With bromide of potassium we obtain crystals of hydrobromate of hæmatin, of a rosy tint. On treating blood with borax and glacial acetic acid, we obtain all the crystals described by MM. Robin and Verdeil in their "Traité de Chimie Anatomique" under the name of hematoidin. Glacial acetic acid alone gives, without the aid of any other reagent, fine crystals of acetate of hemine.

ZOOLOGY AND COMPARATIVE ANATOMY.

Effect of Strychnia on Medusæ.—An interesting paper, describing the development of Medusæ, and the effect on them of certain poisons, is given by Mr. G. J. Romanes, F.L.S., in the Proceedings of the Royal Society (No. 165). He states that strychnia exerts a very marked influence upon Medusæ. "Of the species I have met with *Cyanæa capillata* is the most suitable for showing the effects of this poison, from the fact that, in water kept at a constant temperature, the normal pulsations of this animal are as regular as are those of a heart. Shortly after a solution of strychnia has been added to the water in which a specimen of *C. capillata* is contained, unmistakable signs of irregularity in the pulsation of the animal supervene.

This irregularity then increases more and more, until at last it grows into well-marked convulsions. The convulsions manifest themselves in the form of extreme deviations from the rhythmical character of the normal contractions, amounting, in fact, to nothing less than tonic spasms. It is further of importance to remark that the convulsions are very plainly of a paroxysmal nature—prolonged periods of uninterrupted convulsions being every now and then relieved by shorter periods of repose, during which the Medusa remains perfectly motionless in a fully expanded form. *C. capillata* will live for many hours when under the influence of strychnia, but eventually death supervenes. The animal dies in full diastole."

The Development of the Bird is thus summed up by Mr. A. S. Packard, Junr., in the "American Naturalist":—1. Partial segmentation of the yolk. 2. The embryo develops much as in the bony fishes until the embryonal membranes appear. 3. Formation of an amnion. 4. After the alimentary canal is sketched out, the allantois buds out from it. 5. The avian features appear from the sixth to the tenth day. 6. The embryo leaves the egg in the form of the adult, and like the reptile, is at once active, feeding itself.

How are the Chameleon's Changes of Colour produced?—A very interesting paper on this subject is that of the Rev. S. Lockwood, in the "American Naturalist" for January 1876. He gives the following admirable illustration of the changes of colour. He says:—Supposing through a sheet of block tin many thousands of little pipes were made just to enter. Let them, if you will, be regarded as infinitely small. Call this series A. Now suppose another series in all respects similar and fixed in like manner. Call this series B. It must be understood that the pipes of one series alternate with those of the other series, so that it shall be first a pipe of A, then a pipe of B, and so on in regular order for both series. Suppose again that the A pipes contain green pigment, and the B pipes contain yellow. We will further imagine that each pipe series has a series of muscles which can act upon them. Now laid over the mouths of all these pigment tubes let us suppose a translucent film. Our perforated block tin and its translucent spread, with the mouths of the colour-tubes opening between them, shall represent the *rete mucosum*, or coloured layer of the skin. Suppose now the appropriate muscles squeeze the lower ends of the A series of pigment-tubes, the pigment at once comes up against the almost transparent skin, the colour of which is now blue. Let the muscles relax and the pigment descends into the tubes again. Let the same process occur with the B series of tubes, and the result will be that the skin shows a yellow colour. Not waiting for the yellow pigment to return into the tubes, let the A series be again squeezed, and up comes the blue pigment against the translucent spread. Now everybody knows that a green colour is easily made by a mixture of yellow and blue. Suppose the little spots where the blue touches under the translucent film to be so small as to be called molecules, and suppose the same of the spots where the yellow pigment touches, and you have all the conditions necessary for begetting green. It is also easily imagined how by regulating the amount of muscular pressure the proportions of the separate pigments is regulated, and so the most delicate tints are produced.

Digestion in Insects.—A contemporary states that M. Plateau finds that

when the salivary glands of insects are not diverted from their primitive function to become silk or poison glands, they secrete a neutral or alkaline liquid, possessing, at least as regards one pair, the property characteristic of the saliva of vertebrate animals of rapidly transforming starch matters into soluble and assimilable glycose. The change is effected in a posterior dilation of the œsophagus. At this place results in the carnivorous insects a transformation of albuminous matters into soluble substances like peptone, and in vegetable-feeding species an abundant production of sugar out of the starchy matter eaten. When digestion has taken place in the œsophagus, it is submitted to an energetic pressure in the gizzard or proventriculus, which is armed with teeth. It thus seems that this is not an apparatus for crushing the food, but for expressing the liquid from the food triturated by the jaws. In the stomach, or middle intestine, as Plateau calls it, the food is again submitted to the action of an alkaline or neutral liquid secreted by local glands, present in the Orthoptera, or by a great number of small glandular cæca as in many beetles, or by a simple lining of epithelial cells. This fluid has no analogy with the gastric fluid of vertebrate animals. Its function differs according to the group to which the insect belongs. In the carnivorous beetles it makes an emulsion of the greasy matters; in the Hydrophilid beetles it continues the conversion of starch into glycose, begun in the œsophagus. In the caterpillars of the butterflies and moths it determines a production of glycose and makes an emulsion of greasy matters, and in the grasshoppers no sugar is formed in the intestine, as this material is produced and absorbed in the œsophagus.

The Corals of the Galapagos Islands have been investigated by Count Pourtales, who has published the following account ("Silliman's American Journal") :—The Galapagos Islands are, as is well known, an important point in the geographical distribution of corals, being almost exactly on the boundary of the coral-producing part of the Pacific Ocean, and that portion which is destitute of them on account of the low temperature of the water. All the writers on the subject have placed this group of islands in this latter portion. During the visit of the United States Coast Survey steamer *Hassler*, a number of specimens of corals, of which the following is the list, were picked up on the beaches of several of the islands:—*Ulangia Bradleyi*, Verrill, Indefatigable Island; *Pavonia gigantea*, Verrill, James Island; *Pavonia clivosa*, Verrill, Indefatigable Island; *Pavonia*, sp., James Island; *Astropsammia Pedersenii*, Verril; *Pocillipora capitata*, Verrill, Jervis and Charles Islands; *Porites*, sp. The undetermined *Pavonia* is a massive species with larger calicles than those of the two other ones, and more porous and lighter. The specimen is too much rolled for nearer determination. The *Porites* is massive also, and in the same condition. The species are all, or nearly all, identical with those found at Panama. They are mostly reef-builders, but here live probably isolated and at a certain depth, having never been observed *in situ*. In individual growth they are fully equal to those from more favoured localites, the rolled pieces of *Pavonia* measuring six or seven inches in diameter, thus indicating masses of considerable size originally. They are not confined to the northernmost islands of the group, where we should more naturally look for them, from the greater proximity to the warm current; but, as the list shows, a *Pocillipora* was found

at Charles Island, one of the southernmost. The probability of fragments drifting from one island to the other is very small, owing to the considerable depth of water between them.

The Development of the Ascidian like that of a Vertebrate Animal.—One of the best accounts of the development of ascidia that we have seen in English is that in the "American Naturalist," by Mr. A. S. Packard, Junr. Mr. Packard says—after going into the subject at length, and with the aid of various woodcuts—it will be seen that some highly important features, recalling vertebrate characteristics, have occurred at different periods in the life of the embryo ascidian. Kowalewsky remarks that "the first indication of the germ, the direct passage of the segmentation cells into the cells of the embryo, the formation of the segmentation cavity, the conversion of this cavity into the body cavity, and the formation of the digestive cavity through invagination—these are all occurrences which are common to many animals, and have been observed in Amphioxus, Sagitta, Phoronis, Echinus, &c. The first point of difference from other animals in the development of all vertebrates is seen in the formation of the dorsal ridges and their closing to form a nerve-canal. This mode of formation of the nervous system is characteristic of the vertebrates alone, except the ascidians. Another primary character allying the ascidians to the vertebrates, is the presence of a *chorda dorsalis*, first seen in the adult Appendicularia by J. Müller. This organ is regarded by Kowalewsky to be functionally, as well as genetically, identical with that of Amphioxus. This was a startling conclusion, and stimulated Professor Kupffer of Kiel to study the embryology of the ascidians anew. He did so, and the results this careful observer obtained led him to fully endorse the conclusions reached by Kowalewsky, particularly those regarding the unexpected relations of the ascidians to the vertebrates, and it would appear from the facts set forth by these eminent observers, as well as Metschnikoff, Ganin, Ussow and others, that the vertebrates have probably descended from some type of worm resembling larval ascidians more perhaps than any other vermin type, though it is to be remembered that certain tailed larval Distomæ appear to possess an organ resembling a *chorda dorsalis*, and farther investigation on other types of worms may lead to discoveries throwing more light on this intricate subject of the ancestry of the vertebrates. At any rate, it is among the lower worms, if anywhere, that we are to look for the ancestors of the vertebrates, as the Cœlenterates, Echinoderms, the Mollusks, Crustacea and Insects, are too circumscribed and specialized groups to afford any but characters of analogy rather than affinity.

The Early Stages of the American Lobster.—Mr. S. I. Smith, whose paper on this subject appeared in abstract in a number of "Silliman's American Journal" (1872), has now published the essay in full, accompanied by a series of plates, and has been good enough to forward us a copy. His paper is of considerable length, yet, from the circumstance that he was unable to study the different forms in their living state, none of the internal anatomy is given. Nevertheless, the memoir is of value and will repay perusal, although it extends over too much space to render an abstract possible. We may observe that the terms employed are those which are used by Milne Edwards, Latin being substituted for French.

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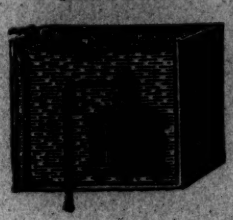

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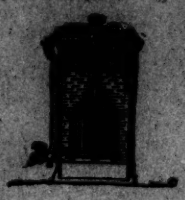

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